VOLUME XXXII OCTOBER, 1941 PART 2

THE NATURE OF SHYNESS[1]

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I. A SOCIAL PHENOMENON

THE phenomenon of shyness has received very little systematic attention(1,2), although it is of special interest in applied psychology, where tester and psychotherapist have to overcome shyness in order to obtain rapport.

Shyness is mainly a social phenomenon, always expressed by behaviour and only in relation to other human beings. A person alone on an island would not be called shy: we might speak of his timidity or modesty, but not of his shyness. The meaning of the word 'shy' is vague (cf. (3), p. 10). We find shyness described as a character trait, as an attitude, or as a state of inhibition. The fact of inhibition as such is not a distinguishing mark, as the inhibition of impulses is commonly expected after the earliest phases of childhood. However, inhibition in shyness is not volitional, but compulsory. It does not further either maturation or civilization, which are the aims of social inhibitions. It is a state of hyper-inhibition, usually accompanied by physical symptoms like blushing, stammering, perspiring, trembling, going pale, accessory movements and increased urinary and faecal urges. The mental state is described by the individual as a feeling of inferiority, of not being wanted, of intruding; it is coupled with an inability to say the right thing at the right moment—while a hundred

[1] Substance of a paper read to the British Psychological Society's Nottingham meeting on 20 April 1941. Part of a thesis on "Play and Therapy" accepted for M.Sc. in the University of Manchester.

youngster as objectively as would his equals in age. Children are accustomed to be judged not according to their actual work done or knowledge shown but with regard to their age level, and in the family circle this judgement is usually emotionally tinged. These instances suggest also that the shy individual has clung to his childishness and is not fully grown up.

As far as shyness towards groups is concerned, it appears that the individual wants to be recognized as a member of the group by its members. If actual recognition is not possible on account of internal or external differences, the individual may still wish to be regarded, by those outside or inside the desired group, 'as if' belonging to the group. The advantages of belonging to a group, especially one admired by the individual, are manifold. He can expect the objective evaluation by the group members towards him to be changed, e.g. he gets from the other members loyalty, privileges and help which decrease his tension and apprehension and make him feel secure. Membership of any group in authority, especially when manifested by a uniform—as the fighting services, police or even bus-conductors—neutralizes individual shyness. A policeman may be shy in private life, but when in uniform he is normally able to control his behaviour and not to appear shy. The uniform helps to attain a standard of objectivity otherwise not reached.

IV. Shyness and boastfulness

When considering the psychology of shyness, we have to deal also with the contrasting phenomenon of boastfulness and bragging. The mental attitude which accompanies this form of behaviour appears to be similar to that in shyness. The outward form of 'blustering self-assertion', as McDougall(5) calls it, is described by many authors as an attempt to overcome one's shyness, to disguise it or to cover it up. In boastfulness as compared with shyness we find a change from passivity to activity; instead of withdrawing we meet forwardness, the aggressive character of which becomes very obvious. The hyper-inhibition of shyness is replaced by a lack of inhibition in boastfulness. The individual does not express feelings of inferiority but assumes an air of superiority. Both forms of social relationship have in common a feeling of great insecurity and the incapacity to show oneself to advantage. Although in boastfulness the contact with others is increased, the result remains mainly negative. The two extremes could be permuted to give four different forms: shy *and* boastful behaviour may both be accompanied by feelings

of inferiority; or outward shyness *and* outward boastfulness may go with an inner attitude of superiority.

Form (1), shy behaviour combined with feelings of inferiority, is commonly called shyness.

Form (2), boastful behaviour which serves as a screen for feelings of shyness.

Form (3), shy behaviour combined with feelings of real or inflated superiority; to this form belong those people who suddenly cease to be shy when they have an occasion to shine and who stay more or less involuntarily in the background. These persons may not be conscious of their desire to be in the centre of attention.

Form (4) shows relatively uninhibited self-display which is outside my subject; it is, however, problematical whether this form exists at all.

V. VARIATIONS OF THRESHOLD IN DIFFERENT NATIONS

Although constitutional differences exist, social influence and education are important in producing shyness. We might say that shyness is almost an effect of education. American surveys(6) have shown that teachers do not consider shyness in school children as a problem, because this type of behaviour does not give them any trouble, as do, for example, open rebellion, lying or stealing. The teachers' attitudes therefore set a standard for the child, who knows that by behaving shyly he is more apt to win praise than punishment.

As shyness is a social phenomenon, it is not astonishing that its threshold varies according to the standards and ideals of the group. This variation can easily be proved by a reference to language, for clearly different nations have different notions about this form of behaviour. The French, Spanish, Italian and Latin languages do not possess a single word—like shy—which covers the whole complex. In all these languages the notion is decomposed into three main factors: fear, shame and hostility. There is only one word in French, *se gêner*, which corresponds closely to the English expression 'to be shy'. The word *gêne* is derived from Hebrew *gehenna* denoting 'hell, a place of torture'(7). This etymology shows clearly how the French temperament judges the inhibitions connected with shyness and social conventions.

Apart from the words *scheu* and *schüchtern*, there is the German word *blöde* which meant originally 'weak, delicate, shy'. During the last 50 years the meaning of this word has changed slowly and *blöde* denotes now only stupidity; this contemptuous judgement has at the same time

influenced the judgement of shyness which is considered in Germany as a form of slight stupidity.

The English language is by far the richest in expressions for shyness, and the English seem to be relatively most sympathetic to it. In a nation whose 'understatement' is a proverbial characteristic, this strong sympathy for shyness is not astonishing. The 'understatement' seems to represent form (3), where inner feelings of superiority are coupled with outward shyness.

The list of synonyms for shyness in different languages shows a preponderance of fear and mistrust and a smaller proportion of shame and embarrassment. It is interesting to note that the feelings of inferiority which play such a great part in the individual's consciousness are not specially expressed in the different languages. There is only the English word 'diffident' which forms a connecting link between mistrust and the feeling of inferiority.

The change from passivity to activity in boastfulness is very clearly expressed by language: instead of the individual himself being shocked, he behaves in a shocking way, instead of being embarrassed, he embarrasses others.

VI. Unconscious background of shyness

As the shy individual is not curtailing his actions voluntarily, we have to assume that unconscious factors are at work in producing the inhibition which seems to have been partially adapted to reality during the individual's childhood. As the lack of inhibition in boastfulness does not allow the making of friendly contacts either, we can safely assume that the instincts, emotions and actions inhibited in shyness are asocial. What does the fear in shyness inhibit? Comparing shyness with boastfulness, it appears that in the shy person aggression is inhibited. A further hint in the same direction is the well-known irascibility of shy people when sudden fits of anger and bad temper break through their usual mildness (8).

A number of synonyms for shyness are derived from words which denote hostility and ferocity. There is further the so-called *esprit de l'escalier*[1] where biting remarks come into the individual's mind when the occasion for uttering them has passed. Clumsiness (13) which is often observed in shy people—regarded as a blunder—may also represent an outbreak of repressed aggression.

Shyness and boastfulness are both at work in the attack arising from fear (*Angstangriff* (9)). Here fear changes into aggression. This fear may

[1] This expression was coined by J. J. Rousseau in his *Confessions*.

be either fear of real events or fear of the strength of one's own instincts, one's own aggression, just as a prison wall is both a defence against people coming in from outside and against escape from within. We may venture to say that the fear in shyness inhibits aggression, and that in boisterousness the fear has possibly increased to such an extent as to necessitate an outbreak of aggression in self-defence.

If we consider the inhibiting factor of shame, we find a very close connexion between shame and sexuality; modesty and self-display. Before the individual shows signs of shame in connexion with moral laws, he is ashamed of his own body, its functions and those actions, thoughts, and words which refer to it. The compromise between shyness and self-display is clearly seen in the symptom of blushing. The blushing person unwittingly attracts more attention, so that a repressed desire for self-display and exhibitionism is gratified in this symptom (10). Comparatively uninhibited self-display comes out very often in boisterousness. Language gives us here an amusing support. The expression 'to strut' which is sometimes used to describe the pompousness of a person shows in its etymology a possible connexion with genital exhibitionism. The word in its original sense means 'to stick out stiffly' (11). The connexion between shame and sexuality is expressed clearly by the English, German, French, Latin and Spanish languages; e.g. till to-day the word *Scham* denotes in German the feeling of shame and the external masculine or feminine genital parts. In English the double meaning of the word 'shame' ceased about the beginning of the seventeenth century.

Connected with both fear and shame is secrecy. If we link it with the original objects of shame, the body, actions and thoughts concerning it, the possible connexion with masturbation suggests itself, all the more when we take into consideration the possible existence of guilt feelings (which may also be expressed through blushing), the pre-occupation with the self and the infantilism of the shy individual. Some of the stereotyped movements and symptomatic actions usually connected with deficient mastery of limbs belong to this realm, e.g. fidgety hand movements of shy people. The connexion of fear and shame with masturbation seems to be due to early educational prohibitions.

While the shy individual is usually aware of his feelings of fear and shame—although he does not know against what these inhibiting barriers are directed—he will usually not admit feeling any mistrust of people. This is not at all astonishing as fear and shame are socially accepted emotions or propensities, while mistrust is socially disapproved. The individual's constant effort to repress the reappearing unconscious

material of aggression and sexuality needs the mistrust as a guardian against outside attempts to penetrate or loosen the barrier. The fact that the shy person's instincts have to be under constant repression and control is the reason why little deflexion or sublimation is possible; but as his instincts are neither deflected nor sublimated they have to be kept under repression and control. This is the vicious circle in which the ego of the shy individual is imprisoned.

VII. Character and personality

With regard to character and personality the shy individual belongs to the group of narcissistic[1] characters with mainly negative and egocentric tendencies, although he may behave in an apparently altruistic manner. The constant guarding and watching leads to the formation of a rigid character which is not flexible enough to embrace new experiences or to adapt itself to new situations and requirements.

The social and emotional immaturity of the shy individual points to a damage in development. There are doubtless possible connexions with different developmental phases of childhood, specially with the questions of masochism, sadism and castration fear. But there is up till now too little evidence to allow more than mention of this. The shy individual seems to behave like an impotent child, longing to be admitted to the group of potent adults (fathers), being at the same time afraid of being too inferior and fearing to be snubbed for his presumptuous wishes. This fear of being snubbed represents in a figurative sense castration fear. Etymology shows also a literal connexion. The verb 'to snub' meant originally 'to snip off the end of a thing'. The Icelandic *snubbotr* means 'snubbed, nipped, with the tip cut off'. 'To snub' is allied to the Swedish *snöpa* which means 'to castrate' (11).

VIII. Neurosis and psychosis

The combination of subjective mental suffering, inability to change this state of mind voluntarily and impaired relations with the environment, is characteristic of psychoneurosis. Therefore we have to consider shyness as a psychoneurotic syndrome. A clear diagnosis of the type of neurosis is not possible. Shyness has in common with conversion hysteria the symptomatology, ambivalence, instability and sexual components; with anxiety neurosis, fear and inhibition; while indecision, mistrust and

[1] Etymology lays special stress on the point that narcissism designates not only the direction towards the self, but at the same time the exclusion of the outer world. *Narcissus* and 'narcosis' have the common Greek root: ναρκόω = I benumb; ναρκάω = I grow numb (11).

aggressive components belong to the symptomatology of obsessional neurosis. The constant watching of the mental state of the shy person seems to run parallel to the constant watching of the organic side of the hypochondriac person. This watching from without—known as depersonalization when reaching a high grade—is found in schizophrenia and also in melancholia. A state of severe shyness may recall schizophrenic attitudes to such a degree as to cause diagnostic difficulties. Many investigators have emphasized the similarities between simple dementia praecox and shyness (1,2,12), but there are also features common to paranoia, e.g. increased references to the self and the exaggerated preparedness to associate and relate outer happenings to themselves.

The change between shyness and boastfulness can be clearly seen in drug addicts; it is characteristic of, for example, alcoholism or morphinism that the individual—usually inhibited and withdrawn—is cheerful and boastful under the influence of the drug.

IX. SUMMARY

Summarizing, it may be said that shyness is a state of hyper-inhibition through fear, shame and mistrust, directed partly against the environment, partly against the subject's own impulses, mainly aggression and sexuality.[1]

Shyness is a psycho-pathological state which shows connexion with psychoneuroses and with psychoses, especially of the schizophrenic group.

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[1] These findings may help towards the understanding e.g. of functional enuresis. A high percentage of enuretic children is shy (about 85 % at the Manchester Child Guidance Clinic); the overcoming of their shyness, that is, the acceptance, recognition and expression (and possibly later the suppression) of their own impulses, specially their aggression, has been shown to have a beneficial influence on their enuresis.

AN EXAMINATION OF THE VALUE OF THE EMPIRICAL APPROACH TO AESTHETICS

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I. *Introduction* (pp. 114–116).
II. *Assumptions implied in an empirical aesthetics* (pp. 116–120).
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IV. *Types of hypothesis related to each class, and their value* (pp. 122–128).
V. *Conclusion* (pp. 128–129).
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I. Introduction

In view of the growing tendency characteristic of modern psychology to regard the study of human behaviour as an exact science, to question the value of any of the vast and ever-growing accumulation of statistical data derived from the mathematical analysis of controlled human behaviour is to invite the criticism that any such doubts are but the product of a die-hard and reactionary conservatism. Whatever may be the value of the statistical approach to general psychology, however, the application of the empirical method to the study of aesthetics has a value sufficiently restricted to justify an examination of its usefulness and of its limitations, at least in this field. A further need for such investigation lies in the intrusion into the literature of aesthetics of views on the value of the method ranging from an irritated contempt for any other form of investigation to the denial of the possibility of an 'exact aesthetics'. Prof. Seashore, in introducing the fourth volume of the *Iowa Studies in the Psychology of Music*, expressed the hope that "the jargon of arm-chair theories will be replaced gradually by an adequate scientific aesthetics" ((1), p. 5). On the other hand, Lord Listowel asks: "Can the surest and most modern of scientific methods, by means of which the conditions of an event are deliberately varied and controlled, a method admirably suited to the investigation of the material world by chemists and physicists, and gaining ground rapidly in the realm of living organisms,

be employed with equal success to elucidate so fragile and evanescent a flower of the mind as beauty?" ((2), p. 165).

The most obvious argument in justification of the method is that the empirically and mathematically educed data provide material from which the aesthetician may construct, or by which he may test the accuracy of, his hypotheses, in the certainty that at least—however nebulous may be his ultimate conclusions—he has something definite on which to build. Unfortunately, he is not always convinced that the carefully elaborated mathematical superstructure is itself based on anything so exact, definite and certain. The class of experiment which is based on relative aesthetic preferences, for instance, examples of which are scattered throughout the voluminous records of empirical aesthetics, is open to suspicion and often in fact suggests more problems than it solves. One such problem is the nature and assumed constancy of the judgement of preference, the unit on which the whole investigation depends. Perhaps it may be permitted to take as a typical example the recently published researches of Eysenck(3).

In one of these, fifteen subjects of ages varying from twenty years to seventy, including artists, university students, bank clerks, typists and teachers, were asked to rank in "order of liking" a number of postcard reproductions of landscape paintings and portraits and a number of photographs of statues and of landscapes, selected "with a view to making them equal in 'goodness'". One immediately wonders on what individual grounds the preferences were made and by what factors of individual experience the choice was influenced. What is the nature, the qualitative content, of the judgements which form the ultimate unit of the mathematical analysis? Can it be assumed to be qualitatively constant, a unit, when the fifteen subjects, carefully selected to be a group representative of the general public, are for that reason so varied in age and in aesthetic experience? In the case of the artists the preferences may reasonably be assumed to be genuine aesthetic judgements and, therefore, related to the 'goodness' of the pictures. But what is this 'goodness'? Is there any relation between aesthetic value and degree of preference as measured by the sum of preferences? It is a matter of common observation that there is no such relation. There are no queues for the art galleries; it is not the symphony which pleases what Dr Bridges termed 'the common ear'; the lyrical gem which delights the lover of poetry has no charm for the multitude. If the degree of popular preference, therefore, has no relation to aesthetic value, what is its value to the study of aesthetics? Moreover, if some of the subjects are expressing a

preference which is related to aesthetic value, while others are expressing preferences influenced probably by associations—as Valentine has shown them often to be(4)—or by other personal tendencies and aesthetically irrelevant factors such as youth, extraversion, or radicalism, as Eysenck has suggested, the unit on which the mathematical analysis is based has no qualitative constancy. The main conclusion, that the more youthful, radical and objectively minded members of society tend in general to prefer the brighter things, including brighter pictures, may be of general psychological interest; but this can hardly be regarded as 'a factor in aesthetic judgements', for it bears no relation to aesthetic value. If all the pictures of the world were classified as bright and dull, few would deny that each category would include every degree of aesthetic value from an immortal beauty to the banal, trivial and counterfeit.

It is clear at least that the value of empirical aesthetics is not self-evident, and that the extent of its usefulness should be investigated. In this paper it has been possible only to examine some of the assumptions on which the possibility of an empirical aesthetics is based, to attempt a classification of the kinds of experiment which can be employed, and to examine the kinds of hypothesis which can be formulated in each of the experimental classes.

II. Assumptions implied in an empirical aesthetics

Aesthetic experience may be defined as that experience which tends to evoke an aesthetic judgement; that is, a proposition that something is, or is not, beautiful. Notwithstanding the extent of individual differences in the perception and judgement of beauty, and notwithstanding the infinite diversity of things so judged, it is clear that the judgement of beauty is a generally intelligible concept. However much one may question a specific judgement of beauty, the meaning of such a judgement is universally understood. It is irrational, moreover, not to assume that this meaning has a qualitative constancy, for the same objects (again notwithstanding their infinite diversity) tend and have always tended to evoke in general the same judgement. We can still find beauty in the cave drawings of Palaeolithic Man, in the surviving poetry of the ancients, even in translation, in the sculpture of the Greeks, and share their delight in them, as we can presume their joy in the sunset and flowers, in colour and form, as we can understand their love of music. It is only by postulating the constant and recognizable quality of the affective state or subjective experience evoked by the objects judged beautiful that any meaning can be assigned to the aesthetic judgement, for it is the only

factor discoverable common to all aesthetic experience. While it is fallacious to argue, as believers in 'significant form' would appear to argue, that all objects which evoke a particular kind of subjective reaction must, therefore, have some other common character, that is, other than their common ability to evoke the response, it is not fallacious to assume that they may have. Empirical aesthetics makes this assumption, and as the objective factor in the aesthetic situation is capable of scientific control, it is possible both to investigate objective data generally recognized as beautiful for the purpose of objective analysis, and to control stimuli selected for investigation in relation to selected and non-selected 'experiencers' or 'subjects'.

Fechner is generally regarded as the founder of exact aesthetics, in that he first systematically propounded as a method the advantages of approaching the problem 'von unten', empirically, instead of 'descending' metaphysically from general concepts, 'von oben', to the particular. Fechner differs, however, from many of his modern disciples in that he did not denounce the metaphysical approach as valueless, but held the view that "a philosophy of aesthetic can be constructed after and over an empirical aesthetic just as a philosophy of nature can be erected on physiology and physics" ((2), p. 40). It is an exaggeration of the scientific exactitude of the empirical method, however, to claim, as has been claimed, that it makes no a priori assumptions ((2), pp. 9–10).

That there are in fact many such assumptions, presumably ignored by the empiricists as self-evident truths, is obvious, for otherwise the experiments themselves have no co-ordinating factor; that these assumptions are not self-evident truths is demonstrable. One such assumption which has prevailed since Fechner's deliberate employment of it is that relative aesthetic values of certain aesthetic stimuli are determinable by the degree of pleasure evoked by them. That this is an assumption the truth of which is not self-evident and is in fact doubtful has already been demonstrated by reference to the observable fact that most people find their greatest pleasure, in this department of human experience, in the spurious and gaudy, the crude and tawdry, the sentimental and imitative, the rhythmic jingle or vulgar ornament. Nor is the assumption made more acceptable when the pleasure is qualified as 'genuine aesthetic pleasure', for the only difference between the enjoyment of a bad work of art and that of a good one is in the work itself—and even critics disagree. The relevant point is that at least one of the assumptions which are fundamental to a large proportion of the experiments of the empirical aestheticians is not a self-evident truth.

Another assumption, though questioned by such careful investigators as Valentine, is that the analysis of the aesthetic response to simple stimuli can be utilized to throw light on those complex forms which are constructed from such simple elements. "Of course we shall find great changes in the effects of colours or lines when they are built up into complex arrangements such as pictures. But we may reasonably expect some of their most striking and characteristic effects to remain even then" ((4), p. 11). While this assumption, thus qualified by Valentine, is almost certainly true, that it is not self-evident or necessarily true is proved by the existence of opposed views. Richards, who denies the truth of the assumption, holds the view that "the more simple the object contemplated, the more varied the responses will be which can be expected from it" ((5), p. 9).[1]

A more important and elusive though allied assumption is that the investigated characteristic is the actual stimulus of the aesthetic response. In such an experiment as, for example, one which involves the use of Zeisung's 'golden section', where a relation is objectively isolated, the assumption appears to be self-evident, though the element of association in stimuli apparently so isolated as single colours makes even such an assumption as this capable of doubt. A different kind of experiment better illustrates the doubtful nature of the assumption. An extraordinary amount of labour has been directed towards the analysis of minute variations from mechanical accuracy in the interpretation of music by solo performers of recognized efficiency and 'artistry' under controlled conditions. The perfection of the methods of phonography has enabled the most minute variations in pitch, time, and intensity of sounds to be accurately recorded and to be graphically represented for investigation(6,7,8). The question arises, however, as to whether the beauty of the work or that of the performance of it is being investigated. Volume IV of the *Iowa Studies in the Psychology of Music* includes a complete diagrammatic record of the First Movement of Beethoven's '*Moonlight*' *Sonata*, as played by Prof. Philip Greeley Clapp. From the record every slight variation in time and intensity is apparent, and it is possible even to see how long the thumb is held down in the arpeggio passages. But does the beauty of the Beethoven fragment depend on these departures from the precise relations indicated in the score? It is obvious that it neither does nor can do. The same quality of interpretation

[1] Prof. Valentine's comment on this view is "Even so (if true), the more likely are they to include all the possible responses to a more complex object, and so to help us to look out for these".

can be applied to a work of any intrinsic quality, and any aesthetic value which the interpretation itself may have is distinct from the aesthetic interest which the work itself may arouse. Moreover, unless there is some common factor discoverable in the variations themselves, as there are common tendencies in the violin and vocal vibrato, such experiments serve no possible purpose except to expose the nature and extent of personal idiosyncrasy. That there is any but a negligible degree of consistency even in the repeated performances of the same work by the same artist has been demonstrated by the experimentalists themselves ((1), pp. 342 sqq.). The mere fact that variation from the regular is a factor which influences aesthetic experience by its tendency to maintain and restimulate aesthetic interest is demonstrable without so elaborate an investigation. One does not need to measure the variations in the rose petal to know that roses would be of less permanent aesthetic interest if, in Robert Bridges' phrase, they were "as similar in shape as all equilateral triangles" ((9), p. 289).

One of the most valuable of the assumptions accepted as a basis for experimentation is that the developed judgement of taste is a cultured derivative from innate aesthetic tendencies in the child and, in a modified form, in primitive man. In the investigation of such innate tendencies and of the kind of influences which in general modify their development, experimental aesthetics has made a valuable contribution to the study of the aesthetic experience.

There are two final assumptions, important because they are tacitly assumed by experimenter and subject alike, and accepted by child and adult, artist and psychologist, with unquestioning certainty. One is that the experience is always the same kind of experience, irrespective of the nature of the stimulus. This may be anything from a coloured surface, a rectangle, a musical interval or a group of rhythmic nonsense syllables to an incomparable work of art in any medium; the investigation may be an analysis of the colour preferences of an infant or of the degree of *portamente* a singer employs in passing from one note to another; in all these and all such investigations it is always assumed as obvious that they are all directed towards the same ultimate purpose and, therefore, that the aesthetic experience is qualitatively constant.

The second of these tacit assumptions is that the phenomenon which is ultimately being tracked down is that of beauty. Prof. Valentine's book on empirical aesthetics is, for example, entitled *An Introduction to the Experimental Psychology of Beauty*. The general acceptance of these assumptions is shown by the fact that no reference is ever made to either

of them. There is no question about the *fact* of beauty, and there is no mystery or doubt, in the general mind, about the *fact* of its equal manifestation in situations of an infinite diversity.

III. Classification of the experimental categories

In any aesthetic situation there can be only three elements: an object, regarded as constituting the aesthetic stimulus; a subject, stimulated into some kind of reaction; and the relation between them. As the investigator can direct his experiment towards any one of these three elements, there are, therefore, three fundamental classes of experimental aesthetics. Külpe's classification differentiates the experiments in respect to the relation between subject and object; if this scheme is used as a basis, and the objective and subjective classes are superimposed on this classification, it is possible thereby to obtain a complete analysis of all the possible kinds of experiment, because such a classification exhausts all possible elements in the situation.

Külpe(10) exhausts the classification of the possible relations between subject and object by dividing the experiments into three categories. His first class is that in which the relation is that of impression (*Eindrucksmethode*); his second is that in which the relation is that of expression (*Ausdrucksmethode*); and his third is that in which the relation is productive or manipulative (*Herstellung*).

In the first class the experiment is directed towards the relative valuation of controlled stimuli in accordance with the degree of aesthetic satisfaction such stimuli appear to be capable of evoking. The second class is that especially used by Valentine, and is based on the subjective analysis of mental states which a specific experience tends to evoke in different subjects. From this class, for example, are derived all the experimentally produced data from which are deduced the hypotheses which formulate such influences as that exerted by association on the aesthetic experience. The third class provides material which the subject himself can control and manipulate for the production of an aesthetic end. The necessity of eliminating skill and specific knowledge, not only because they are essentially irrelevant factors but more especially because their presence disturbs the control of the experiment by destroying subject equality, restricts the utility of this class of investigation. It is, however, a valuable kind of experiment and one capable of considerable development, if sufficient caution is exercised in its control and in the analysis of the evidence furnished.

It is obviously possible to extend this classification by grouping the

different kinds of 'stimuli' employed and by grouping the different kinds of subjects employed. There are four kinds of 'objects' available for such experimentation, and of these the fourth is a specific variety of the third. It is possible to use, first, a simple sensory stimulus, such as a colour, a line, a sound, a scent, or a surface intended to be touched. It is possible, secondly, to use two or more of such elements in controlled relation, as in experiments with musical intervals or linear relations. A point, subsequently developed, is that this class of experiments assumes that a *relation* between two simple sensory elements or 'fundaments' may have aesthetic value, as the former class assumes that the simple apprehension of relatively isolated sentient experience may reveal such value in the fundaments themselves. An isolated sound, like the note of a flute, may be judged beautiful, and a relation between two sounds, a musical interval or 'chord' may be so judged and preferred to another interval in musical relation, though the aesthetic value of the sounds in isolation remain constant. Thirdly it is possible to use works of art as objective 'stimuli', and, lastly, reproductions of natural phenomena, such as photographs and sound records. In these two classes the relations are, respectively, aesthetically purposed by the artist and achieved by Nature, and in neither case subject to experimental control other than that of selection.

Logically this exhausts the possible objective variants, but the transient nature of sound as opposed to the relative permanence of visual art introduces a confusing factor into the psychological study of the art of music. Except where there exists the rare ability to extract any aesthetic satisfaction from a music score, and assuming, as is obviously true, that music is intended to be heard, music differs from the visual arts in that its presentation to the 'subject', the listener or the contemplator, depends on an interpreter who is not necessarily and is but rarely the creator of the work. Thus, while the sculptor and the painter leave for posterity their own work for direct and immediate contemplation, the musician leaves but a record of symbols for future executants to reconvert into the sounds from which its aesthetic value has to be judged. Thus, a fifth class of objective data may be used, the records of 'performed' musical works, differing from the products of visual art in that the interpretative modifications of the performer are superimposed factors in the valuation of the work.

The true relation between the intrinsic value of a musical composition and its interpretation by a soloist or by an orchestra, or some other group of executant musicians, is clear if the same relation is sought in the world of the poets. Poetry, like music, may be recorded symbolically, but it

differs in that the ability to read literary symbols silently, and to appreciate the value of what is read, is much more general than is the ability to read silently, and to appreciate by that process, a music score. But it would be absurd to pretend that the beauty of a poem depended on the inflection of the voice in reading or on any other quality incidental to its actual interpretation by a reader, or, as the Iowa School of empirical psychologists claim for a music score, that the written work is 'only a schematic reference' about which the interpreter weaves his own imaginative interpretation.

It is possible to use the third and fourth classes of objective material either in relation to a subject or group of subjects, as the first and second classes must be used, or, their intrinsic beauty having been assumed, they may be objectively analysed for possible common and constant characteristics.

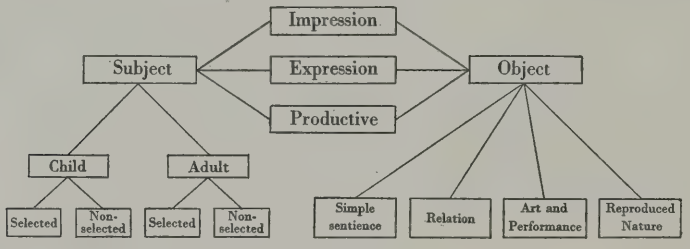

Fig. 1.

The final classification is that which is based on the selection or control of the subject. Here again it is easy to exhaust the possible categories. The most valuable division is that of children and adults, and either may be 'non-selected', and therefore, if sufficiently numerous, presumably normal or typical of general childhood or of the adult public; or they may be selected so that the group may represent a specific class. The subjects may, for example, be of a given age, a given sex, or environment, or standard of intelligence; the adults may be selected artists, performers, critics, undergraduates, trained psychologists, or what not.

It may be permissible to represent this triple classification graphically, as in Fig. 1.

IV. Types of hypothesis related to each class, and their value

It is neither possible in a paper of this scope, nor relevant to its purpose, to refer to more of the actual experiments than those which illustrate this classification or which furnish data as to the kind of

hypothesis which is derivable from each of the classes. Typical of the Impression method, the method of differentiating controlled stimuli in relation to their relative aesthetic value as judged by their tendency to impress the subject, are the experiments of Féré(11), Winch(12), Valentine(13), Bullough(14,15) and Martin(16), who have investigated the preferences of children and adults in simple sensory stimuli such as colour and line. Similar preferences for specifically related stimuli in such concepts as rhythm, symmetry, balance, musical intervals and linear relations have been investigated by Myers(17), Valentine(18), Ethel Puffer(19), Tennant(20) and Lowery(21). The Expression method takes different forms, but the most obviously typical form is that in which the subject is asked to express a reason for a stated preference, as in Prof. Valentine's association experiments. Other interesting experiments, typical of this class, have been made by Tennant(20) on colour contrast, by Bugg(22) on consonance judgement, by Cicely Parsons(23) on children's interpretations of ink-blots, and by Feasey(24) on children's appreciation of poetry.

What kinds of hypotheses have been or can be made from the data discovered from such investigations? And what is the relation of such hypotheses to the fundamental problems of aesthetics?

The first kind of hypothesis is that which relates to the problem of general aesthetic value, and a valuable hypothesis of this class is that the possibility of aesthetic preference tends to be accepted as a fact without question. Irrespective of the nature of the objective stimuli employed, irrespective of the age or type of subject, whether child or adult, the possibility of evaluating such stimuli aesthetically in accordance with the degree of pleasure experienced in response to them, is a possibility which is invariably understood, and the question is regarded as a perfectly natural one. The very fact that this truth has, so far as I know, escaped special comment is significant, as further evidence of the general tendency to accept the assumption as so self-evident a truth as to be unworthy of comment. But, as has been previously suggested, the implied assumption is not a self-evident truth; nor is the hypothesis a negligible one. If the investigation were restricted to the first class of experiments, the hypothesis could be stated in the form of the proposition that A is intrinsically more beautiful or less beautiful than B (for it has already been assumed that the phenomenon investigated is that of beauty, and that the evoked experience is qualitatively constant). The reasonable consistency shown in the order of the 'aesthetic preference' of such stimuli as musical intervals or colours suggests the truth of this

proposition. But it is not the whole truth. It ignores the possibility that such quantitative differentiation may be determined, or at least influenced, by subjective factors.

The second class of investigation furnishes evidence of the existence of many such factors, which tend to influence and to modify the aesthetic judgement. The most important work of this class of investigation is the establishment of general principles of quantitative aesthetic interference by subjective tendencies or influences. Many examples of such generalized hypotheses may be found in the records of relevant experiments. Prof. Valentine writes, for example, that "Associations may occur with all persons at all stages of aesthetic culture, but a frequent use of them as the main basis of the appreciation of a picture. . . seems to be indicative of a low degree of aesthetic development, at least in this department of art" ((4), p. 86). "If the aesthetic experience is to be enjoyed to the full, there must be no rude interruption in the course of the mental apprehension of the object" ((4), p. 97). "Constant repetition generally produces boredom and disgust, except, perhaps, in the case of the very finest work of art" ((4), p. 112). This is an interesting hypothesis when brought into relation with the apparent paradox "Familiarity is an essential element of the idea of beauty" ((4), p. 111).

The hypothesis that the judgement of aesthetic value is modifiable by subjective, psychological influences suggests that no objective character can, alone, be accepted as evidence of beauty. As Kant expressed it, propositions of the form "All objects which have such and such qualities are beautiful" have no universal validity. Beauty can neither be created nor judged by reference to an objective formula; but any established hypothesis which generalizes the modifying tendency of some psychological or subjective influence is valuable knowledge to the aesthetician, the artist and the critic. Such a proposition as "No objects which have such and such qualities are beautiful" could clearly have universal validity, for though awareness of beauty can never depend on rational judgement, reasons as to why an experience is not beautiful may be deducible, and would be capable of general formulation in the terms of the principles which the second class of experimental investigation seeks to establish.

The kind of hypothesis which can be evolved from the purely objective analysis of works of art has all the dangers of the first class of investigation, that of the relative aesthetic value of general objective stimuli, with the additional dangers due to the fact that art is but a department of aesthetics. The one quality common to all works of art is that they are

designed, in the sense that they are deliberately purposed and controlled. Design, therefore, may be regarded as a constant quality of art, even when the element of form is least apparent, as in a landscape painting or a portrait. A genuine work of pictorial art, even though it achieve an apparent realism, is a deliberate composition, constructed as a synthetic unity from selected and modified elements from the artist's experience, and can never be a merely mechanical and imitative reproduction of Nature. Music is a structure of formally related sounds, and a musical composition must achieve formal unity. Both these facts are true of poetry too and, when translated into their respective media, are true of all arts. From Aristotle's 'unity in variety' to Clive Bell's 'significant form', this quality has been regarded as a characteristic, or as the characteristic, of objects regarded as beautiful, or at least of works of art, for it is difficult to find 'significant form' in Nature. But any attempt to postulate any doctrine based on formal characteristics, or to define beauty or even a specific art in terms of form, always fails and must always fail. They must fail because there is no such valid proposition as "Objects which possess such and such qualities are beautiful". To judge a work of art as beautiful, or successful, because it is consistent with some formal principle, is to make a rational and not an aesthetic judgement. Moreover, it is all too easily demonstrable that many works which have formal unity are not beautiful, and that relative aesthetic value is not proportionate to the degree of formal significance in art. There is a great deal more formalism in a patterned wall-paper than in a mezzotint by Frank Short, but a miraculously conceived formalism could never achieve an equal beauty.

To say that the beauty of a work of art does not depend merely on its formal structure is not to say that its beauty is not influenced by its formal structure. Lack of unity may destroy beauty, though alone it is not sufficient to create it.

Paintings, poetry and music have been used in the second class of experiments, that is, for the investigation of the influences affecting the evoked experience of children and adults representative of many different environments. A most interesting and illuminating comment, incidental to Margaret Bulley's experiments with pairs of pictures, similar in subject but of contrasted aesthetic value, was made by a boy 'subject'. He said, "It is quite easy to choose. All that I think ugly I say are art, and all that I think beautiful I say are not art" ((25), Introd. p. viii).

This frank confession summarizes much of the truth which the mass of evidence furnished by Miss Bulley's and similar experiments tends to

confirm. The words "All that I think ugly" and "All that I think beautiful" perfectly express the spontaneous, individual and 'noegenetic' quality of the aesthetic judgement and the existence of what has often been called the 'intuition of beauty'. The comment also expresses, however, the great divergence between this innate ability which has created the universally intelligible concept of beauty, and the developed appreciative judgement of art. It is this modifiability of the aesthetic judgement which explains the impossibility of assessing aesthetic value of any but the simplest kinds of stimuli in terms of a generally evoked pleasure.

That the appreciation of art demands both a general cultural development and specific knowledge and experience is evidenced by the extraordinary irrelevance which characterizes the influences confessed as reasons for the judgements. Reminiscent experiences and associations of a purely personal nature, however much they may be allowed to intrude into an individual contemplation of a work of art, are obviously unrelated to the beauty of the work or, to express it differently, to its general aesthetic value. Yet the results show that in the majority of cases the judgement is influenced by personal and irrelevant associations and prejudicial dispositions ((25), Appendix I and Supplement).

This may, to some extent, be due to the nature of the experiments, which sometimes ask for a reason or, when no reason is demanded, suggest that it may be offered. Valentine has observed the difficulty experienced by many of his subjects in assigning a reason for their approved choice, and the relative ease with which it is possible to explain disapproval ((4), p. 70). This is true, of course, because a genuine aesthetic judgement is never a rational judgement but, as has already been shown, the truth of this does not deny the possibility of criticism.

More encouraging is the evidence, and it is considerable, that genuine aesthetic judgements do occur even with very young children. The child who writes "There are lovely curves in it", or a boy of six who writes "The sky is nice", is making a genuine, unspoiled aesthetic judgement, very different from a preference made "because I like to play cowboys" ((25), pp. 86 and 87).

The modifiability of the aesthetic judgement either as the result of purposive training or through the accidental influences of environment does not imply that a developed judgement (or a spoiled judgement) is consciously influenced by relevant or irrelevant knowledge, but that the tendency to find aesthetic satisfaction in specific situations is itself capable of change.

Finally, this class of investigation has shown that this modifiability is, in part, a natural development like growth. Evidence for this is furnished by the consistency of the 'age-appreciation' curves for children drawn from widely differing environments, evidence made more conclusive by the curious nature of the curve itself ((25), pp. 88 and 92). The group testing of thousands of children, from England, France, Cyprus and Japan, showed a consistent tendency for the appreciation curve to *fall* from about the age of six years to that of nine or ten years, and subsequently to rise. In one group only, where specific training had been given, the subsequent rise was consistent and rapid to the age of fifteen years, but in this case, too, the same preliminary drop occurred. It has been suggested that this "decline in taste that is so marked as the child grows older is probably due to the fact that the child is then outgrowing its life of fantasy and imagination, and is entering into a phase of extreme realism when it is powerfully attracted by everything practical" ((25) p. 85). It is inevitable that the use of things and their causes and effects should tend, in varying degrees but increasingly, to divert the child's interest from their 'attractiveness' and proportionately to replace an interest which has no utilitarian or rational significance. Unfortunately the original aesthetic tendency is not always fully revived.

The third class of experiments, the productive or manipulative class, presents a fundamental difficulty of procedure. Unless the materials supplied for manipulation are so simple as to restrict their capacity for aesthetic readjustment below the limit of usefulness, the influence of skill and experience enters to destroy the control of the experiment. A further difficulty is how to use the resultant products. How and by what criteria are they to be relatively evaluated? What evidence could they furnish sufficiently reliable for the deduction of any general hypotheses?

An interesting experiment which went far towards a solution of the first problem was made by C. V. Mackenzie, who used as a medium the soft 'rubber' bricks employed in certain building operations. Each member of a boys' art class was given a brick and a number of simple tools, and told that there was a head inside the brick if he were capable of finding it. The results, which I saw, were of an astonishing variety, displaying deliberately purposed conceptions and considerable ingenuity in the adaptation of accidental results (26). The task showed that many of the actual difficulties characteristic of the actual craftsmanship involved in the art of sculpture had revealed themselves, a fact which must have influenced the boys' subsequent attitude to sculpture, where these difficulties had been solved. While such experiments as this play an

important part in the training of the craftsmanship of an art and incidentally influence the appreciative attitude to an art, they afford little evidence of value to the aesthetician. An artist must be a craftsman but a craftsman need not be an artist. The relevant skills have to be acquired and the nature of the medium has to be mastered before the process can be made deliberately subservient to a specific end; and, when this degree of efficiency has been acquired, the end need not be an aesthetic one, nor, when aesthetic, need it be successfully accomplished. In the stage of experimentation the interest inevitably is directed towards the manipulative process and activity, even, to a large extent, when the process is one of simple patterning.

The difficulties and limitations of the manipulative and productive experiments are still greater in music, though the processes are equally valuable as training. Even in the most practised medium of all, that of language, the effort to write poetry before the ability to make a rational and adequate expression in prose has been acquired may be useful training, but it can display little beyond the existence of individual differences in ability and aesthetic sensibility. Even a prematurely developed creative ability and craftsmanship in a particular medium does not necessarily postulate the development of an appreciation of art values in other media, which demand for such appreciation a similarly developed experience in the specific art itself or a similar degree of relevant knowledge.

V. Conclusion

Empirical aesthetics has, therefore, a great value to the general aesthetician, but a restricted value. It can analyse, by the control of simple stimuli and by the employment of child subjects, the fundamental, undeveloped, genetic principles on which aesthetic development is based; it can establish, as generally true, the influences and tendencies which quantitatively control and modify this development and thereby provide useful data for aesthetic training; by an extension of this method it can supply the critic with norms which might help to explain in certain cases the failure of a specific work to evoke aesthetic satisfaction; when a singer or instrumental soloist has 'performed', it can say exactly what has taken place and provide statistics from which possible consistencies in the interpretation of music could be analysed. But it can supply no single rule whereby the artist can create that which is beautiful, and no single principle by which beauty can be judged.

It is only the exclusive claims which have been made for 'exact aesthetics' which makes the realization of its inevitable limitations

necessary. It is difficult to understand why such exclusive claims should be made for this science and for no other. Psychology does not claim that the concept of ethical goodness is the fantastic idea of some armchair philosopher or that the concept of truth is a nebulous hypothesis, yet both these descriptions have been applied to those theories of beauty which have not been evolved from the laboratory. Science is necessarily restricted to the synthetic systemization of observable occurrences or, as in the case of psychology, to observable tendencies. Scientific laws and principles are generalized interrelations of observable events. Science can say what might be expected to occur under certain controlled conditions, because of the observable consistency of interrelated events, but it can offer no reason as to why this should be so. An exact science is subject to the additional restriction of being limited to the use of measurable data, but this, its most exact particular, further illustrates the limitations of science, for all measurement is relative to some arbitrary, comparative or merely convenient unit, or, as in the case of the measurement of value, to the degree of satisfaction relative to a specific end. As motives, ends and, therefore, values, have no place in a pure science, which ignores the use to which its conclusions are applied, the question arises as to what is measurable, other than in terms of value, in aesthetic experience. Any possible measurement as applied to objective data can be only in units of relative degrees of difference in some common attribute—as in the analysis of fluctuations of pitch in musical performance. Any possible measurement as applied to the subject or subjects can be only in terms of the expressed degree of pleasure evoked by the controlled stimuli. This is hardly 'exact' aesthetics, for it is dependent on a unit that is vague, inconsistent, and subject to the influence of non-measurable quantitative factors. It is, moreover, a unit which is applicable only to elementary simple sentience and which is meaningless when applied to any developed art form, for here recognized aesthetic value is demonstrably unrelated to a measure of popular preference.

Notwithstanding the claims of the formalists and the hedonists, it is almost self-evident that beauty is indefinable in terms of objective characteristics or in terms of pleasure; yet, to be wholly comprehensible within the confines of an exact science, it must necessarily be so defined.

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(*Manuscript received* 31 *March* 1941)

THE SPEED FACTOR IN PERFORMANCE TESTS

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I. *Introduction* (pp. 131–132).
II. *Cube construction times* (pp. 132–134).
III. *Kohs block designs times* (p. 134).
IV. *Two speed-factors* (p. 135).
References (p. 135).

I. Introduction

In a recent publication of the Scottish Council for Research in Education(2), in discussing the existence and nature of a speed factor among performance tests, I mentioned (on p. 41) certain further enquiries which I then hoped might be carried out by one of my students. The war has prevented this, and I am therefore writing this short note to report some calculations I have myself made,[1] and the tentative conclusions I have come to, regarding the speed factor.

The performance tests were those given to 443 boys and 430 girls by Dr A. M. Macmeeken(1), and were, in addition to a Binet test, the following: (1) Seguin Form Board, (2) Manikin, (3) Stutsman Picture, (4) Red Ridinghood Picture, (5) Healy Picture Completion II, (6) Knox Cube Imitation, (7) Cube Construction, and (8) Kohs Block Designs. The children were all, or practically all, of those born in Scotland on four days in 1926, and they were tested individually when their ages were round about $10\frac{1}{2}$ (ranging from 8 : 11 to 11 : 9 in the extreme cases). Age has been eliminated from all the correlations quoted below, by partial correlation. The performance tests were given in the above numbered order, at one sitting (the Binet test later on the same day). For the Seguin Form Board times only were taken, and in this test the child was incited to speed and had three tries to see how quickly he or she could finish. The other tests were scored on the quality of the product, but times were also taken (except in the Knox test), and usually moves were also recorded. Since almost all the children produced perfect scores in Stutsman, and a very large majority did so in Manikin, I used times and not scores for these tests in my analysis. For the other tests (nos. 4–8) I used the scores.

[1] With considerable assistance from Miss Gullen, Mrs Youd, Miss Mellone, Miss Murray, Mr Nandi, and Mr Lawley.

Correlations between times and scores I called positive if the short time went with the high score, i.e. speed with good quality.

I extracted four factors, but felt doubtful of the statistical significance of the third and fourth. The first factor was general, and the second, with which I am here concerned, differentiated between the group of three tests where times were used, and the group of five other tests where scores were used. By rotation it could be given the form of positive loadings in the three times, and zero loadings in the five scores. To check this I then added Manikin *scores* to the analysis, expecting this new variate to cluster with the other five scores; but to my surprise it went (both in boys and girls) with the three times. At this point I said (op. cit. p. 40): "I am inclined at present to think that this speed factor, if it be a speed factor, is not so much due to using times as variates, as due to the actual incitement to speed which was employed in administering test 1, the Seguin Form Board.... In the other tests, although times were taken, there was no insistence on speed; but I fancy that in tests 2 and 3 (they were given always in this order) the memory of that demand for hurry might still persist, and show itself in the performance, while in the later tests it might have died away, so that possibly even their *times* may show no clustering with variates 1, 2 and 3." It is this which I have now tested.

II. Cube construction times

I chose first the last test but one to be given, Cube Construction (avoiding the last test of all, Kohs Block Designs, merely because its times were given on the record cards in ten parts which would have to be added together, whereas the Cube Construction times were only in three parts), and had grids made and correlations calculated for Cube Construction *times* with each of the other variates, including age which was then partialled out.[1] The resulting partial correlations are as follows:

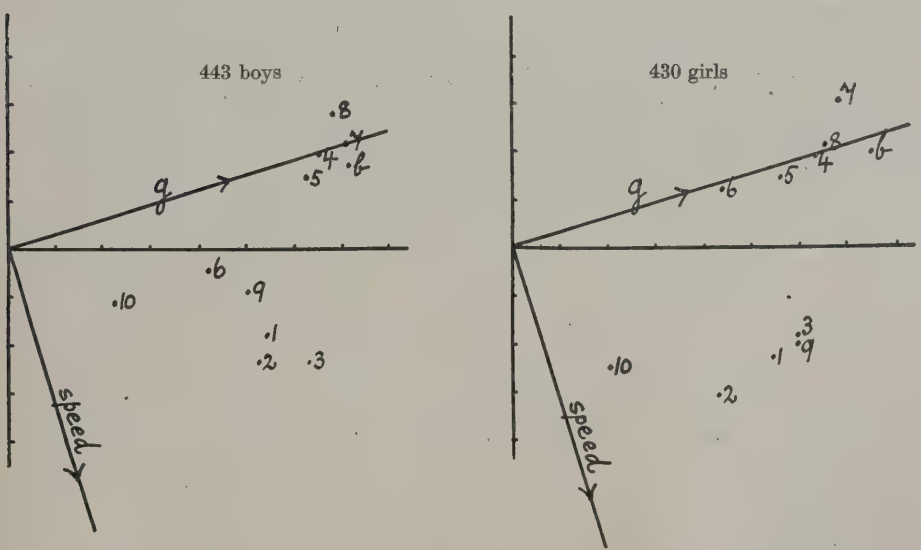
10. Cube Construction times with	443 boys	430 girls
b. Binet I.Q.	0·1266	0·0902
1. Seguin times	0·1552	0·1679
2. Manikin times	0·1245	0·1820
3. Stutsman times	0·1331	0·1799
4. Red Ridinghood scores	0·1404	0·0854
5. Healy P.C. II scores	0·1393	0·0155
6. Knox Cube Imitation scores	0·1327	0·1256
7. Cube Construction scores	0·0237	−0·0376
8. Kohs Designs scores	0·2902	0·2070
9. Manikin scores	0·1047	0·0731

[1] I now regret that I used the abbreviation C.C.T. for Cube Construction Test in my book, for the T. may be misread as 'times'. In that book the variate no. 7, C.C.T., is Cube Construction *scores*.

These may be added to Table V on p. 22 of *An Analysis of Performance Test Scores*. As is explained earlier, these correlations are reported as positive when short time, i.e. speed, goes with high score. None of the correlations is at all high (Kohs alone topping 0·2) and it is striking that there is no correlation between Cube Construction scores and Cube Construction times. (The correlations between Manikin scores and times are 0·3097 boys, 0·3565 girls; see (2), Table V.)

From the enlarged Table V two centroid factors were then extracted, with the following loadings:

	Boys		Girls	
	Factor I	Factor II	Factor I	Factor II
b. Binet I.Q.	0·7183	0·1836	0·7503	0·2053
1. Seguin times	0·5610	−0·1934	0·5605	−0·2452
2. Manikin times	0·5352	−0·2507	0·4544	−0·3084
3. Stutsman times	0·6369	−0·2541	0·6034	−0·1938
4. Red Ridinghood scores	0·6551	0·2001	0·6315	0·1990
5. Healy P.C. II scores	0·6302	0·1574	0·5723	0·1402
6. Knox Cube Imitation scores	0·4346	−0·0493	0·4369	0·1326
7. Cube Construction scores	0·7059	0·2246	0·6826	0·3029
8. Kohs Designs scores	0·6803	0·2810	0·6545	0·2111
9. Manikin scores	0·5185	−0·1002	0·5953	−0·2087
10. Cube Construction times	0·2415	−0·1223	0·2105	−0·2563



It is at once seen, from the signs, that the variate Cube Construction times, in spite of the late position which this test had in the sequence of tests and its distance therefore from the incitement to speed given in test 1, nevertheless has some tendency to go with the other times rather than with the scores. In the diagrams a rotation to factors which may

be '*g*' and 'speed' is indicated. The new variate 10 (Cube Construction times) shows a higher saturation with speed in the girls than in the boys, the difference being of doubtful significance.[1] The boys' diagram might perhaps be taken as in agreement with the hypothesis that an urge to speed given in test 1 had died away in the later tests, but hardly the girls' diagram. However, perhaps the most definite fact is that Cube Construction times (10) are only very slightly saturated with '*g*', and that both the two first centroid factors together account for only a very small portion indeed of the variance of this variate, which is largely independent of both of them.

III. Kohs block designs times

To check all this I turned to the last test to be given, Kohs Block Designs, where up to ten different times had to be added to get the total time corresponding to the score, and I had caused this to be done and grids to be made before my attention was drawn to a peculiarity of the times in this case which made them quite unsuitable for my purpose. There were ten sub-tests of increasing difficulty, and it was Dr Macmeeken's custom to stop the test if two successive sub-tests produced no score. The total times therefore were for varying numbers of sub-tests from two to ten, and an artificial correlation is thus produced between a low score and a short time, which by our convention we call a negative correlation.

It may very well be that children who are clever at making the Kohs Block Designs in exact imitation of the pattern are also quicker at it; but this positive correlation between ability and speed, if it exist, is here entirely overwhelmed by the above artificial correlation. The measured correlations between times and scores were (for constant age):

Boys	−0·8124
Girls	−0·8524

The situation was further complicated by the fact that there were time limits, which were often reached, for each sub-test, and the complication was so great that any attempt to arrive at a 'true' time (by, say, adding the time limits for the sub-tests not attempted) had to be abandoned as quite out of the question.

[1] It is difficult, if not impossible, to test the significance of centroid loadings scientifically since guessed communalities are used. But a comparison of the present diagrams for boys and girls with one another, and with the figure given in (2), p. 39, gives some idea of the accuracy of placing of the dots.

IV. TWO SPEED FACTORS

I now returned to the Cube Construction test data, lest on investigation any difficulty of the same sort should be found there. This test was in three parts, separately timed, and these three times had been added together to give the times used as a variate. As, however, in practically all cases the child had tried all three parts of the test and had been given a score and a time for each, the above difficulty due to breaking off in the middle of the test did not occur. But it is a striking fact that the Cube Construction test data showed no correlation between times and scores, holding thus an intermediate position between the Manikin test on the one hand, where that correlation is positive (+0·3), and the Kohs test on the other, where it is negative (−0·8). It seemed possible therefore that the Cube Construction test was an example where the true positive correlation between ability and speed was exactly balanced by a false negative correlation, produced in some more insidious way than the obvious one observed in Kohs Block Designs, and it is my present opinion that this is indeed the case. There are, it would seem, two influences making for short time, even when the whole test is carried out by every child. The one influence makes one child soon get the correct, or at least a very good, result. The other influence makes another child soon satisfied with any result, even a bad one.

In tests (1) Seguin Form Board, (2) Manikin, and (3) Stutsman the task was easy and the great majority of the children accomplished it perfectly, though at different speeds. But in the Cube Construction test the task was difficult and the performances of the children produced scores spread over a wide range. In those first three 'easy' tests the former of the two influences or factors is at work almost alone. In the difficult test both factors are at work. Some children get short times because they are quick at putting the cubes correctly into position. Others get short times because they are easily and quickly satisfied with a poor shot at the task. If this surmise is correct, it seems to follow that times can only be useful data if they are all times for performing *correctly*, or at least nearly correctly, the task set. Times for performing a task with varying degrees of correctness spread over a wide range seem to be a complex variate of little use in practice.

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(*Manuscript received* 2 *May* 1941)

DARK ADAPTATION DURING STIMULATION WITH COLOURED LIGHT[1]

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I. Introductory

Most of the work described in this paper was undertaken at the suggestion of Prof. James Drever; the main original purpose was an attempt to find means of enhancing normal powers of dark adaptation. Two lines of approach seemed possible: (1) through preliminary stimulation of one or both eyes with coloured light, following upon some unconfirmed results reported by other investigators and discussed in the present paper; (2) through supplementing the diet with vitamin-rich substances, as described in many studies of abnormal cases; it is hoped to describe this

[1] Based in part on a paper read before the Scottish Branch of the British Psychological Society, Glasgow, March 1941.

work in a second paper. The original plan was later partly abandoned and partly expanded to cover a number of cognate topics, which are discussed in the present paper or elsewhere.

II. APPARATUS AND TECHNIQUE

Perhaps more than any other perceptual situation, dark adaptation must be regarded as susceptible to influence from the apparatus and mode of observation employed. This point will be fully discussed elsewhere, but meanwhile it should be remembered that the form in which results are here presented is largely determined by the experimental conditions, and is not necessarily ideal.

Throughout, dark adaptation has been measured in terms of readings on the adaptometer devised and described by Nagel(8). In essence this consists of a light-source illuminating a milk-glass test-field from behind. Light intensity is controlled in two ways: (1) by the interposition between the source and the field of one or more of three reducing plates each of which cuts down the intensity to one-twentieth of previous value; this arrangement gives four 'levels' or ranges of intensity, corresponding points on which represent light intensities in the ratio 8000 : 400 : 20 : 1. (2) Continuous gradation within each range is obtained by means of a diaphragm, controlled by a handle which can be operated either by the subject or by the experimenter. The instrument is so arranged that the four ranges of intensity overlap to a certain extent. Thus, it is possible, by putting in another plate, further to reduce to one-fifth of its own value an intensity corresponding to the starting-point of the next range of intensities. This is a characteristic of the instrument which may have its effect on results, particularly since one of these overlaps coincides closely with a commonly observed point of inflection of curves of adaptation. The total range of effective intensities given is not less than the ratio $10^6 : 1$. A scale, illuminated from below with red light, allows the experimenter to take readings without in any way interfering with the subject's dark adaptation.

All observations were made in a light-proof room with walls painted dull black. Except on a very few occasions, sessions took place between 2 and 5 p.m., lasted for 2 hr. (or a few minutes more or less), and followed 10–15 min. light adaptation to illumination from a 100 W. blue daylight bulb reflected from a matt white surface. The reflector, which was constructed by Mr W. Fulde, of the Heriot-Watt College, Edinburgh, was made of cardboard supported by wooden struts, and was approximately parabolar in section, the interior dimensions being: height 18 in., width

25 in., depth 14 in. An adjustable wire cradle supported a cardboard lamp-shade containing the 100 W. lamp and pierced with holes to allow for ventilation and reduce temperature. The subject's chin rested on a pad of sponge rubber mounted on a wooden block, in such a way that the reflector entirely covered the field of vision. The illumination obtained varied somewhat at different points on the surface; in the portion normally covered by the subject's gaze it ranged between 35 and 45 f.c. This reflector combines ease in handling and cheapness with a minimum of discomfort for the subject. (In the earlier stages of this work a similar but simpler device of white paper mounted on boards was used.)

Originally it had been intended to study mostly monocular adaptation, but since the subjects reported some degree of discomfort under these conditions binocular observation was substituted. Another change which was considered desirable was to reduce the test field from its original form of a disk 4 in. in diameter, as it was found that when this was used idioretinal light caused considerable confusion. Accordingly a disk of light 1 in. in diameter was substituted, and it was found that greater confidence resulted. Since it was desired to study peripheral adaptation only, and to exclude any element of form perception, it was not practicable to use a field of more unmistakable shape, such as the triangle used by Allen & Dallenbach (1).

As finally arranged, the test field subtended a retinal angle of about 6°, and was displaced nearly 40° from the fovea. To ensure constancy in this latter respect, the subject fixated a spot of red light about 1 mm. in diameter, placed at eye-level. The handle controlling the diaphragm was operated by the subject, the instructions being to reduce the intensity until the test field just disappeared, and then to increase it until the light was just visible. This second point was taken as threshold value. In cases of doubt the subject was permitted to check his setting by further adjustment. Readings were generally taken at 5 min. intervals during the first hour and at 10 min. intervals during the second hour, though no attempt was made to adhere rigidly to this plan. Longer intervals were used during the second hour because it was found that at this stage the subjects experienced considerably greater difficulty in setting the field at threshold, and consequently took longer to do so. Time values quoted, however, always refer to the time at which the test light was switched on.

All the experiments were carried out with two subjects, alternating as experimenter:

A. Miss Florence MacNeill, research student and part-time assistant

in the Department of Psychology, whose willing co-operation is gratefully acknowledged, and

B. The present writer, lecturer in Psychology in the University of Edinburgh.

Subject A has normal vision; subject B has congenital amblyopia of the left eye.

Following Nagel's practice, results were generally dealt with by drawing curves in which the ordinates represent reciprocals of threshold intensities (in the present case logarithms of reciprocals). These produce a rising curve, representing increase in sensitivity. Enhanced dark adaptation, if and when it appeared, would accordingly be seen as a further rise in the course of the curve. I have consistently interpreted my results in terms of logarithmic units, since most investigators are now agreed that this is desirable as being in accordance with the general response characteristics of the organism. A fuller consideration of the whole question of recording and interpretation of results will be given elsewhere.

Since differences occur both in rate of adaptation and in what may be termed its 'extent', i.e. final value of threshold stimulus intensity, it seems desirable to note both. *Extent* is here measured in terms of maximum level reached by a smoothed curve of adaptometer readings during a 2 hr. period, excluding rises at the end due to a single high reading. *Rate* is measured in terms of minutes taken for the smoothed curve to reach a given level or levels of sensitivity corresponding to a stated number of logarithmic units above the initial intensity of the adaptometer light. As mentioned later (§ V (4)), this smoothing of curves is not an entirely legitimate procedure, but it is useful or even unavoidable if one wishes to note results in the way just described.

III. Dark adaptation following preliminary stimulation

(1) *The work of Kravkov and Semenovskaja*

As noted in §I, the immediate stimulus to the experiments here described was the results of an earlier series of investigations, those of Kravkov and Semenovskaja[5,6,9]. Since the last of these papers provided the starting-point of the present study and since it is somewhat inaccessible to readers in this country, a brief account of its main features may be given here.

In the earlier papers[5,6] it was claimed that monocular sensitivity in the dark-adapted state was enhanced by stimulation of the other eye,

either before dark adaptation commenced or during its progress. The instrument used was devised by Kravkov, and may be described as a simplified form of the Nagel adaptometer. Three subjects took part in Semenovskaja's experiments(9), all female, with normal vision, and between the ages of 21 and 27.

In general, the procedure was to stimulate one or both eyes with white or red light of 250 or 75 lux (about 23 and 7 f.c.) reflected from a white surface. A period of dark adaptation of approximately 2 hr. followed, readings being taken at intervals of from 3 to 15 min. Each subject carried out two or (usually) more sessions under each set of conditions. The results are presented in the form of (*a*) tables averaging the results for each subject separately, and (*b*) curves, in which the results for all three subjects are pooled. The results are quoted in units proportional to reciprocals of successive threshold intensities. Especially when presented in graphical form they show a very marked enhancement of adaptation (*a*) with preliminary red as compared with preliminary

Table 1. *Summary of results obtained by Semenovskaja*(9) *with and without preliminary stimulation with light of* 250 *lux*

Conditions	Sensitivity		
	At 10 in.	Maximum	At 120 in.
Preliminary red	4·90	5·68	5·04
Preliminary white	4·03	5·50	5·08
No preliminary	4·26	4·97	4·75

white stimulation, and with both as compared with sessions in which no preliminary stimulation was given; (*b*) following stimulation with light of 250 lux (of either colour) as compared with light of 75 lux. The differences are particularly marked for binocular adaptation.

As presented, these results are very striking. Sensitivity values are plotted on an arithmetic scale, and in order to allow all three curves to be plotted on the same axes in manageable proportions, units have to be chosen in such a way that the 'normal' curve seems hardly to rise at all, while the curves for preliminary stimulation soar far above. If, however, the curves are replotted on a logarithmic scale, in the same way as those in the present paper, the differences are seen to be very much less striking. A summary of some of the results is given in Table 1. Each figure is an average of the results of all three subjects.

The actual figures of course depend on the characteristics of Kravkov's adaptometer, and on the whole they range somewhat higher than those obtained in my own series of experiments. It is interesting, however, that the initial values (not here quoted) are very similar to those ob-

tained under comparable conditions by my subject A, who falls within the same category as regards age, sex, and visual characteristics. This would suggest that Semenovskaja's subjects appear to have achieved a somewhat further degree of adaptation.

(2) *Experiments on adaptation following stimulation with red light*

Even logarithmic plotting, however, leaves an impression of marked enhancement of dark adaptation following preliminary stimulation with either red or white light. (A brief discussion of this phenomenon is given in § V (2).) It was accordingly decided to attempt to confirm these results, either in detail, or, if this should prove impossible, in a more general way.

Table 2. *Adaptation with and without preliminary red stimulation* (*Numbers in brackets indicate order in which the sessions were taken*)

Conditions	Maximum sensitivity attained	
	Subject A	Subject B
No preliminary		
1st session	4·47	4·30
2nd session	4·45	4·37
3rd session	4·51	4·46
Preliminary red		
Type (*a*)	4·50 (2)	4·36 (2)
Type (*b*)	4·43 (1)	4·42 (3)
Type (*c*)	4·48 (3)	4·37 (1)

Various types of red stimulation were tried:

(*a*) A white surface, as described on p. 138, viewed through red spectacles. For subject A a 40 W. white milk-glass bulb was used; for subject B a 100 W. blue daylight bulb. Semenovskaja's method was of this type.

(*b*) The same surface, illuminated by a 25 W. red Osram lamp, and viewed directly.

(*c*) The red-illumined surface, as in (*b*), viewed through red spectacles, as in (*a*).

Each subject did one set of observations with each type of red light, and by way of control, three sessions 'without preliminary stimulation'—except for 5 min. adaptation to the general illumination conditions of the test-room, which was lit with an ordinary 60 W. opaque bulb about 7 ft. above the floor. This represents a slight departure from Semenovskaja's practice, which involved 50 min. preliminary adaptation to a given illumination, but the completely negative character of my results makes it plain that the small differences in technique described are unlikely to have been of significance.

Table 2 summarizes the results of the sessions with and without

Fig. 1. Dark adaptation with and without preliminary stimulation with red light. Average curves.
——— with preliminary red ---------- no preliminary

preliminary red stimulation. That no effect was produced by stimulation with red light is still better seen in Fig. 1,[1] in which the results for all types of red stimulation are averaged for each subject, and compared with average curves for the sessions without preliminary stimulation.

(3) *Experiments on adaptation following interruption with red light*

A further variation of the same technique was tried as follows: Following 15 min. light adaptation, the subject was dark-adapted until adaptation seemed virtually complete; this took 75–80 min. Ten minutes stimulation with red light was then given, and a further series of readings taken during the remainder of the usual experimental period; this amounted to 25–30 min. The results are summarized in Table 3.

Table 3. *Summary of adaptation results before and after interruption with red light of two intensities*

		30 min. immediately preceding interruption		Period following interruption		
Subject	Relative intensity of red light	Mean sensitivity	Final reading	Initial reading	Mean sensitivity	Final reading
A	50	4·36 ±0·039	4·37	4·34	4·37 ±0·020	4·35
A	1	4·27 ±0·044	4·35	4·28	4·23 ±0·078	4·24
B	50	4·42 ±0·063	4·55	4·20	4·39 ±0·064	4·43
B	1	4·38 ±0·070	4·52	4·47	4·40 ±0·073	4·49

Since adaptation was in general still proceeding during the 30 min. preceding interruption, direct comparison of the dispersion of sensitivity measures before and after interruption cannot be made. It is interesting, however, that in three cases out of four this dispersion is as great as or greater than that caused by the series of rising readings in the fore-period. This fluctuation is in agreement with the observation of Lythgoe & Phillips(7) that any change of conditions during an experimental period brings about irregularity of readings. The other facts that emerge are that the drop in sensitivity caused by the interruption is small, and that after about 30 min., if not before, dark adaptation has nearly if not quite reached its former level. No significant or regular difference seems to exist between the effects of the two intensities used.

(4) *Conclusions regarding preliminary stimulation*

General conclusions from all these experiments seem to be as follows: (1) There is no evidence of the enhancement of dark adaptation through stimulation with red light noted by Semenovskaja. (2) A certain im-

[1] In all figures ordinates represent logarithmic 'sensitivity units', zero being the maximum intensity of the adaptometer.

provement in sensitivity, corresponding to ordinary dark adaptation, does, however, occur *during* such stimulation. In other words, adaptation to some extent takes place under these conditions. As will be seen from the initial portions of the curves in Fig. 1, this occurs more rapidly than in the presence of even fairly dim diffused artificial light, and the results in Table 3 show that the loss in sensitivity ensuing from stimulation with red light while in the dark-adapted state is relatively small.

IV. Dark adaptation during continuous stimulation

The above conclusions led to the study of the main subject of this paper: how far it is possible to achieve normal dark adaptation during continued exposure to coloured light. This is a matter of considerable practical interest, and in order to obtain information on conditions which might conceivably occur in practical situations, two different types of light stimulation were tried, neither being that of the previous experiments: (1) diffused coloured light of various intensities, from a source above and behind the subject, and (2) 'point' lights from an 'Ever-ready' cycle lamp, placed in the subject's field of vision, in a position approximating to that of a motor dashboard. In all cases the light was kept on throughout the session, but was switched off to enable the subject to make settings on the adaptometer.

(1) *Diffused light*

Two colours were originally used in the first series of experiments—the red lamp already used in the experiments described in §III (2) and (3), and a similar blue lamp. The intensity of the stimulating light was varied, as in the case of the 'interruption' experiments, by means of a rheostat, the extremes being in each case in the ratio 50 : 1. These intensity values are of course relative to one another only for a particular colour; i.e. intensity '50' (the maximum intensity for each lamp) for the red light had not the same brightness or illumination value as intensity '50' for the blue. The relations of the various intensities of the different coloured lights is discussed below, and in §V (3*a*). Later, one subject (B) did a series of three sessions with orange light.

Detailed results are given in Table 4 below. In Fig. 2 curves of composite results for each subject are shown. These represent the average of results obtained with two intensities ('50' or '25' and '1') for subject A, and with three ('50', '10' and '1') for subject B. The two sessions with blue light of intensity '1' (see Table 4) are themselves averaged in obtaining subject A's curve for blue, and the anomalous result obtained

in one of the sessions with red light of that relative intensity is omitted. This result, noted in square brackets in Table 4, was obtained from a session which took place after a break of about 7 weeks during the summer vacation, during which time no experiments were carried out. Effects of practice, or lack of practice, were also observed in other connexions, and will be discussed elsewhere. For a time I also thought that subject B's results for orange light might have been affected by the fact that these sessions took place at a different phase of habituation to the test situation, but the results for red light noted in Table 4 are almost identical with those of the first 2 hr. of a prolonged session with red light of the same intensity ('50')—described in another paper(10) and not

Table 4. *Extent and rate of adaptation during continuous stimulation with coloured light. Rate measured in minutes to reach sensitivity levels* 2, 3 *and* 4

Colour	Relative intensity	Extent		Rate	
		Subject A	Subject B	Subject A	Subject B
			Diffused light		
Red	50	4·40	4·25	4– 8–25	6– 9–46
	10	—	4·28	—	6–11–26
	1	[4·39]	—	[4– 9–33]	—
		4·67	4·41	4– 8–16	4– 8–19
Blue	50	—	4·18	—	4–11–54
	25	4·32	—	4–15–29	—
	10	—	4·13	—	4–11–40
	1	4·54	4·31	4–11–18	5–11–27
		4·66	—	3–11–19	—
Orange	50	—	4·38	—	6–10–30
	10	—	4·43	—	6–10–24
	1	—	4·54	—	5– 8–17
			Point lights		
Red		4·60	4·44	4– 9–18	5–11–28
Green		4·38	4·31	4–10–30	6–12–47
Yellow		4·38	4·20	4–10–33	6–13–53
			Ordinary conditions (average)		
		4·65	4·47	4– 8–17	5–10–22

included in the table—which took place much closer in time to the sessions with orange light.

Without particularizing for the present, it may be said that extent and rate of adaptation show a good degree of inverse correspondence with intensity of stimulating light of a given colour.

Comparing the curves, however, it will be seen that both subjects show the same slightly better degree of adaptation with red as compared with blue light. It will further be seen that B's results with orange show still more complete adaptation. These results provided something of a surprise, since the red and orange lights were considerably brighter than

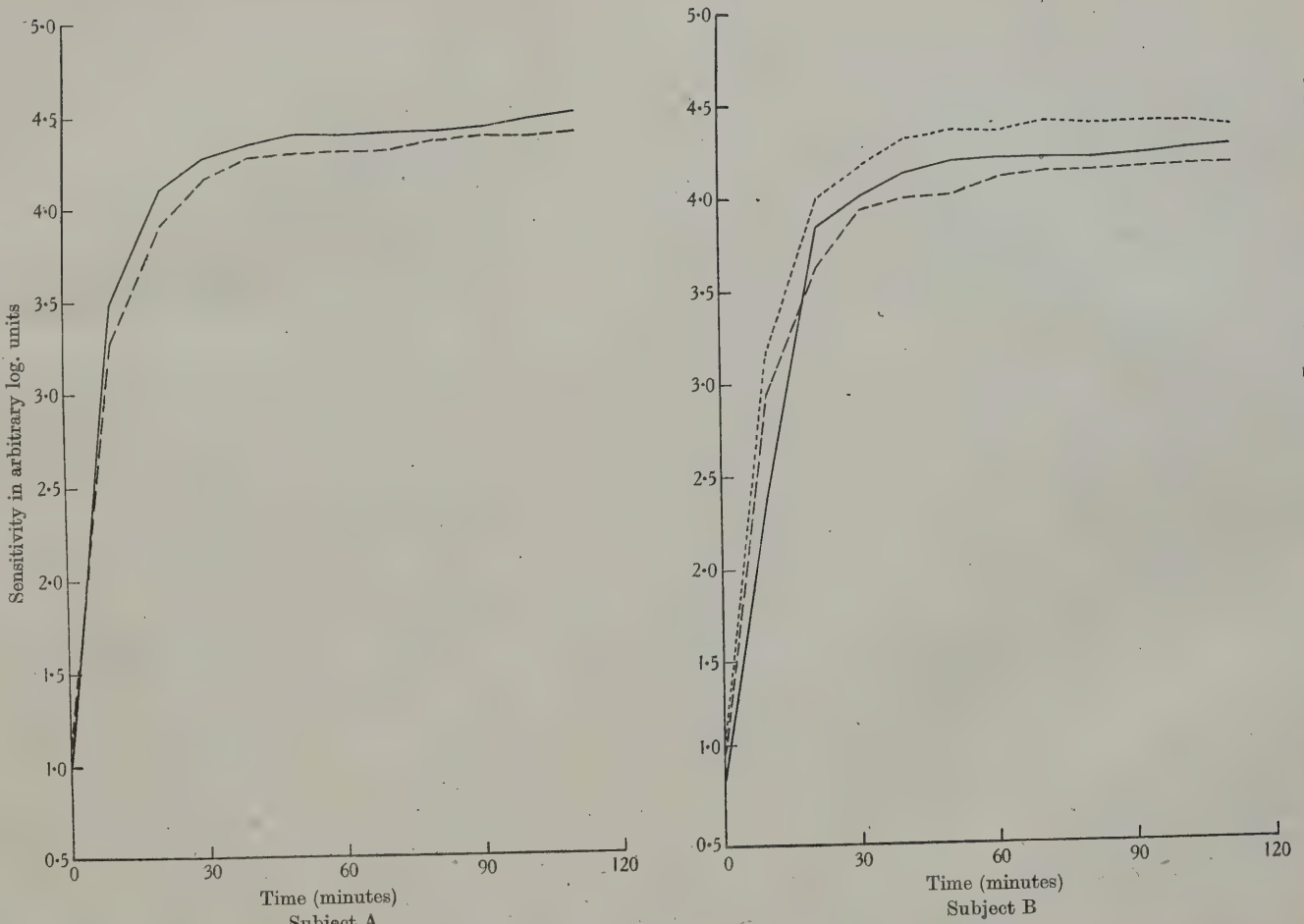

Fig. 2. Dark adaptation during continuous stimulation with diffused coloured light.
——— Red — — — — Blue ---------- Orange

the blue of each corresponding 'intensity value'. It must therefore be concluded that in addition to intensity a colour factor is operative. An attempt to evaluate this in terms of the relative visibility factor is described in §V (3*a*), but the possibility of the influence of affective conditions induced by the various colours must also be borne in mind.

(2) *Point lights*

Somewhat different results were obtained with the point lights. The colours were obtained by covering a 2·5 bulb with gelatine confectionery wrapping. The polished metal reflector of the lamp was removed, and only a slight degree of reflexion from a hemispherical dark surface remained. The subject was not obliged to look at the lamp all the time, but was instructed to keep his gaze in the general direction of the light. Three colours were used for each subject: red, green and a different yellow for each subject.

Results are quoted in Table 4, but a better idea of the actual course of adaptation will be obtained from Fig. 3. It will be seen that for both subjects red gave the most complete adaptation, then green, then yellow. Once again factors other than simple intensity may have been operative, and the degree of agreement between the results for the two subjects is as striking as for the diffused light.

(3) *General conclusions regarding stimulation with coloured light*

The quantitative results obtained with both types of light may be summarized by saying that for all the conditions studied a degree of adaptation not far short of normal was obtained. A more precise statement is difficult to make, in view of the pronounced day-to-day variations observed. Maximum sensitivity during the test-period, however, calculated in terms of the logarithmic sensitivity measure used throughout this paper, was never less than about 90% of normal. Rate of adaptation, on the other hand, seemed to be somewhat more affected, especially when the brighter stimulating lights were used. It also seems to be a less stable characteristic of the adaptation process (see Table 4).

V. DISCUSSION

(1) *Psychological interpretation of the dark-adaptation process*

Throughout my work I have used the term 'dark adaptation' with a purely psychological meaning, viz. the increase in sensitivity to light which occurs when the observer is subjected to conditions of darkness,

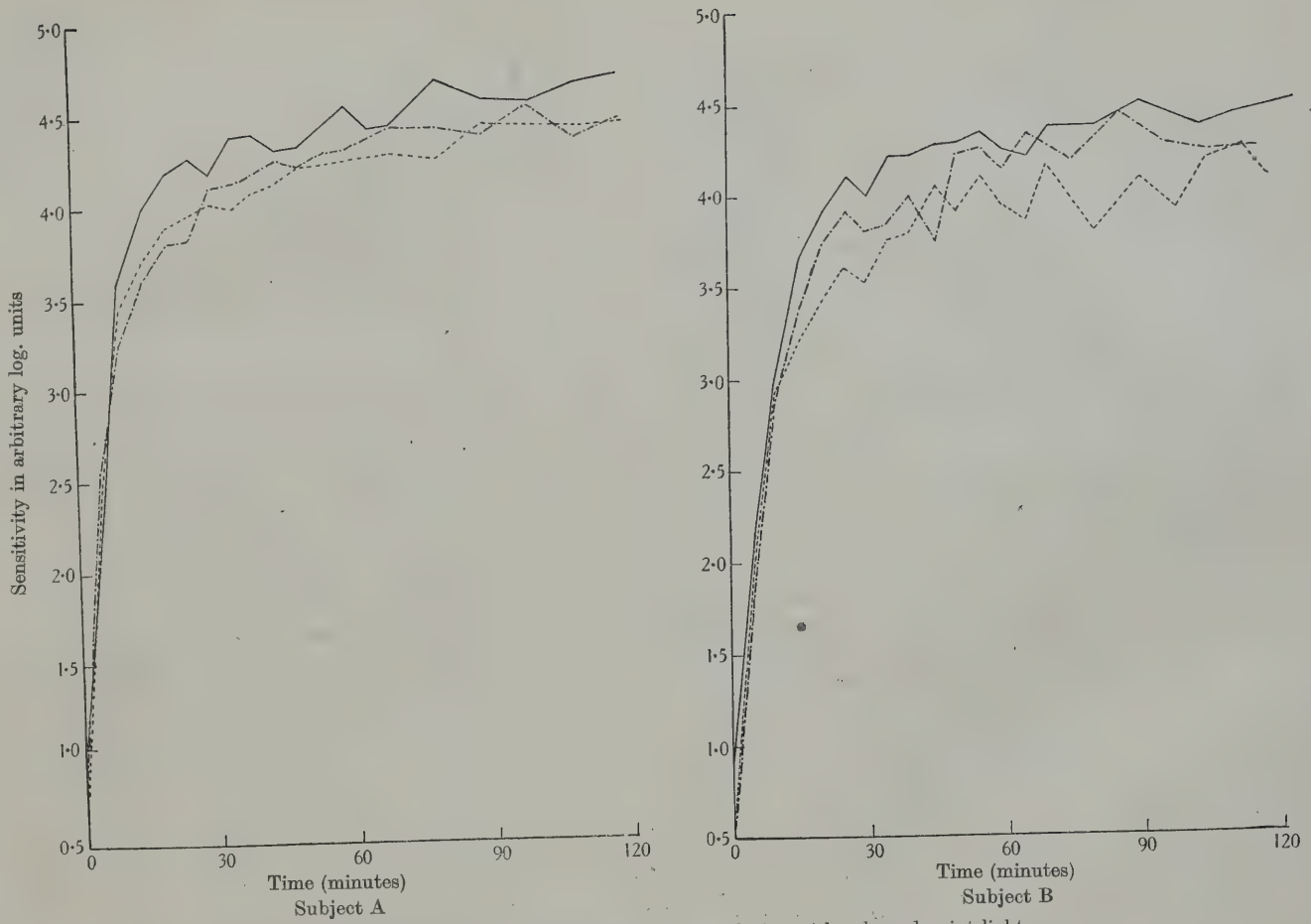

Fig. 3. Dark adaptation during continuous stimulation with coloured point lights.
———— Red ---------- Yellow —·—·—·—·— Green

'dark' being defined, as in Warren's *Dictionary of Psychology*, as "characterized by low brilliance, low illumination, or low brightness". With this definition, then, one can legitimately speak of dark adaptation as occurring during conditions of incomplete darkness or low illumination, such as those described in this paper. Besides being regarded as a process, dark adaptation may also be considered as a state, complete dark adaptation being the end-state corresponding to maximum retinal sensitivity. Considered in this way, there can be objection to the concept of fluctuations in the dark-adaptation process, or that of falling off towards the end of an observation period. My experiments have yielded instances of both these phenomena, the former in almost every session, however short, the latter only occasionally, and not to the extent noted by Kravkov & Semenovskaja ((6),(9), and see Table 1). In fact, as noted elsewhere(10), it was found that subject B was able to maintain dark adaptation unimpaired for a period of at least 6 hr., and there seems no reason to believe that this should not apply to any practised observer. A full discussion of this and other subjects raised in this section will appear in another paper.

Following the definition quoted, therefore, the present discussion is not primarily concerned with theoretical considerations which might furnish a physiological explanation of the phenomena observed. A certain amount of fairly convincing interpretation on a psychological basis may however be put forward.

(2) *The effects of preliminary stimulation*

Such classical authorities as Aubert and Nagel are quoted (by Allen & Dallenbach(1) and others) as expressing the view that dark adaptation is facilitated by the occurrence of light flashes, a fact which they claim can be readily verified in ordinary experience. Although this claim lacks experimental confirmation, there is a sense in which such facilitation does seem to take place, since, as shown by Allen & Dallenbach(1), Suchman & Weld(11) and others, temporary decreases in sensitivity caused by the interruption of the adaptation process by means of light flashes are compensated in such a way that the curve catches up, as it were, on itself, so that the time taken to reach a given level of sensitivity is the same as it would have been had no interruption occurred. This would explain the crossing of the curves of adaptation with and without 'preliminary stimulation' (see Fig. 1), but there appears to be no evidence, other than the results of Kravkov and Semenovskaja, of dark adaptation proceeding so much further following any sort of stimulation

with white or red light. Present-day practice, indeed, seems to assume that no effect is produced, since practically every method of measuring dark adaptation includes a preliminary period of light adaptation. What evidence there is, e.g. in the work of Hecht, Haig & Chase(2) and of Johanssen(3), suggests that, contrary to Semenovskaja's findings, a brighter preadaptation light produces, if anything, impaired rather than enhanced adaptation.

(3) *The effects of continuous stimulation*

(*a*) *The relations between colour and extent of adaptation.* As regards the results obtained for dark adaptation during continuous stimulation with coloured light, it seems possible so far to make only the broadest of generalizations (cf. §IV (3)), to the effect that fairly complete adaptation is possible under the conditions described, and that this 'completeness' seems to be of the order of 95%, this being an average figure for both subjects and all the conditions studied.

Both for theoretical reasons and for practical purposes a fuller study of a wider range of colours and intensities would seem to be necessary, and with it a further investigation of the 'recovery period' noted in §V (3*b*) below. On the other hand, an extended investigation of this sort would inevitably necessitate a very large number of sessions, with a corresponding degree of care in eliminating effects of practice and day-to-day variation. I therefore decided to present the results so far available for the sake of their immediate interest, and to try to account for various apparent discrepancies.

The relative intensities of the different coloured lights were assessed by means of a Ferranti photometer, with the results given in Table 5. Values in foot-candles are not given, since in the case of the diffused light there seemed to be no particular point in the room at which measurements, if made, would be of particular significance, while in the case of the point lights the light was insufficiently strong to give accurate readings in the normal position of the subject's head. It will be seen that while for the point lights completeness of adaptation varies inversely with the intensity of the respective colours, the opposite holds good for the diffused lights. Since, in the words of Kaye & Laby(4), "a given amount of energy is differently evaluated by the eye according to its wave-length", it was thought that taking account of the relative visibility factor of the different colours might reduce the results to greater order.

A rough spectrometric analysis of the colours used was therefore

carried out, and values of the relative visibility factor for the mid-point of each range obtained by interpolating in the data of Kaye & Laby's table (4).

These values are noted in Table 5. I did not consider it worth while at this stage further to analyse the colours in order to determine the relative brightness values of the different portions of the various spectra, all of which proved to be wider than originally expected, although this should really have been done in order to assess the brightness effects of the different colours *in the dark-adapted state*. In particular, the extension of the spectrum of the green light to the red end displaces its mid-point practically into the region of yellow, and consequently raises its visibility factor considerably.

An alternative method of assessing the colours, suggested by Dr K. J. W. Craik, was also tried. This consisted of determining relative legibility of print under each of the conditions of illumination studied.

Table 5. *Characteristics of coloured lights used. Intensity figures relative to one another only within each group*

Colour	Relative intensity	Wave-length range in $\mu\mu$	Relative visibility	Relative legibility
		Diffused light		
Red	3·5	618–688	0·089	71
Orange	5	561–698	0·38	61
Blue	1·2	447–524	0·17	217
		Point light		
Red	8	585–723	0·087	77
Yellow B	16	522–684	0·59	59
Yellow A	21	503–667	0·82	56
Green	11	491–635	0·98	67

A rough comparison was made, with the mean results quoted in Table 5. These values, expressed in seconds taken to read one page of homogeneous material with the lights in position as in the adaptation experiments, give some idea of the relation between the two types of light (maximum intensity was used for the diffused lights). High figures of course indicate poor legibility, and it will be seen that it was barely possible to read with the blue light.

It appears that no mode of combining any of the figures quoted with the observed intensity values makes much difference to the quantitative relationships between the colours. None of these observations, therefore, suggests any ready explanation of the non-correspondence between intensity or brightness of the diffused light and interference with the adaptation process. The only possibility seems to lie in some effect of the extreme wave-lengths at the red end, since the diffused orange light

extends further in this direction than the red. The effect is absent in the case of the yellow point light for Subject B, which was a definite orange-yellow. Otherwise one can only fall back on explanations in terms of variability or of the purely subjective conditions mentioned elsewhere in this paper.

(*b*) *Rate of adaptation.* Rate of adaptation under the different conditions studied also varies, but less attention has been paid to this point, since the technique of the present experiments does not seem to allow of high reliability in measurement of rate. As has been mentioned, reversals of direction in the adaptation curve occur frequently and appear to be both unavoidable and of genuine psychological significance. But since rate of adaptation can be measured only in terms of time taken to reach an arbitrarily selected level corresponding either to a given intensity of the test field, or, more correctly, to a given number of logarithmic units above initial sensitivity, and since this can be done only with reference to a smoothed curve, it is plain that irregularity of occurrence of reversals may seriously influence the point at which the curve reaches the stated level. The effect is least noticeable in the early stages of the adaptation process, where reversals are rare, but it has been found in these experiments that measurement in the early stages does not discriminate well between sets of observations which in the later stages show considerable divergence. This is perhaps due in part to difficulties of accurate timing, but it appears also that during adaptation a change takes place in the phenomenal appearance of the threshold intensity, and that its perception is affected by slight after-image and dazzle effects produced by the stimulating light. Under conditions of continuous stimulation both subjects reported that especially in the later stages the test field took a few seconds to appear, but thereafter was seen as clearly as under normal conditions. Whether this is to be regarded as a rapid return to normal adaptation following a lapse due to the stimulation is not quite certain. Unfortunately it is not possible to allow the subject to make his observations while the coloured light is actually on, since this would alter the situation to the estimation of a difference instead of an absolute threshold. Accordingly it is impossible to draw conclusions regarding the subject's sensitivity while actually under stimulation.

(4) *Affective elements*

A final point which is probably of some significance is that different types of stimulation may well induce general affective states which may have some influence on the adaptation process, or, more properly, on the

adaptometer readings. It is a well-known fact that sensory stimulation may affect discrimination in other sense modalities, although there is some doubt as to the way in which this is brought about. If, as I am inclined to think, it is through the emergence of a more or less general affective condition, it seems possible that the greater effectiveness of the red (and orange) light, particularly of the diffused type, may have been due in part to its more 'cheerful' quality as compared with the blue, as well as to its other qualities already discussed. Controlled experiments on the effect of mood on sensory discrimination are of course difficult to arrange, but general observations suggest that positive affect favours discrimination, and that negative affect tends to have an inhibitory effect. (The former effect has often been claimed to have been demonstrated as a result of listening to music. In the present series of experiments gramophone selections were sometimes played between readings on the adaptometer, though no actual readings were ever taken while the music was being played; statistical analysis failed to reveal any effect whatever.) Although in some respects a minor point, this is one which might have considerable practical significance, and the difficulty of making adequate allowance for it provided another reason for not at present attempting further investigation of other colours.

VI. SUMMARY

1. Two main topics are discussed in the present paper: (1) the possibility of the enhancement of dark adaptation by means of preliminary light stimulation, and (2) the degree to which dark adaptation is possible under conditions of higher illumination than is generally understood by the term 'darkness'.

2. Observations were carried out over periods of 2 hr. with two skilled subjects, using the Nagel adaptometer. Since the choice of instrument and of testing technique has a considerable influence on the results, comparison with the findings of other investigators is possible only in the most general terms.

3. No evidence was found confirming the claim that sensitivity in the dark-adapted state can be enhanced by preliminary stimulation with red light.

4. Adaptation during stimulation with coloured light of (*a*) diffused and (*b*) 'point' types was studied. It was found that for the same colour of light extent of adaptation varied with the intensity of the stimulating light. No such correspondence was found as between different intensities

of different colours. It is suggested that in addition to relative visibility factors certain affective influences must be taken into account.

5. Throughout the investigation stress has been laid on the psychological aspects of the dark-adaptation process; i.e. the subject's experience of the test situation. It is shown that in addition to variations in extent and rate of adaptation, individual differences occur in respect of the regularity of its course. Results may also be considerably affected by practice and by day-to-day variation.

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(*Manuscript received* 23 *May* 1941)

EXPERIMENTALLY INDUCED OLFACTORY PARAMNESIAS

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I. Introduction

We described in an earlier paper(1) a method of inducing paramnesia under experimental conditions. It was shown that visual material given under hypnosis, and associated with post-hypnotic amnesia, evoked a paramnesic variety of recognition when given on the following day in the waking state. These experimental paramnesias closely resembled the states of consciousness commonly associated with incomplete or arrested recognition. It was suggested that they threw some light on the origin of paramnesia in daily life.

We stated in our earlier paper that odours as well as pictures were used in the experiment. Odours were chosen in preference to auditory or tactual stimuli largely on account of the frequency with which they are said to be associated with paramnesia in daily life. We anticipated, of course, that it would be more difficult to obtain conclusive results with

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the olfactory stimuli than with the visual. In the first place, most subjects (unless specially trained) experience great difficulty in recognizing odours after the lapse of a few days, and practically none in identifying pictures. In the second place, as we have already said, paramnesias are commonly aroused by odours in the course of everyday life, and must be expected from time to time in any experiment involving their recognition.

In this paper we describe the results obtained from the experiments with the odours, confining ourselves to three of the five subjects who participated in the enquiry. The degree of amnesia aroused for the remaining subjects was so slight as to render the results worthless so far as the present paper is concerned. Their results have been indicated sufficiently in our earlier paper.

II. Method of experiment

(1) *Material*

Twelve odours in all were used. These were divided into three sets:

Series AO (three odours) given on the first day of the experiment;
Series BO (three odours) given on the following day during hypnosis;
Series CO (six odours) given on the third day of the experiment along with the previous stimuli, *AO* and *BO*.

Series AO: *AO*, 1: Nitrobenzene; *AO*, 2: Valerian; *AO*, 3: Aniseed.
Series BO: *BO*, 1: Capronic acid; *BO*, 2: Creosote; *BO*, 3: Thymol.
Series CO: *CO*, 1: Vanilla; *CO*, 2: Oil of Juniper; *CO*, 3: Civet.
CO, 4: Camphor; *CO*, 5: A commercial perfume.
CO, 6: Verbena.

No special consideration was given to the question of the degree of similarity between these various stimuli.

The *order of presentation* of the odours on the third day was always:
(1) *CO*, 1; (2) *AO*, 3; (3) *CO*, 2; (4) *BO*, 1; (5) *CO*, 3; (6) *BO*, 2; (7) *AO*, 1; (8) *CO*, 4; (9) *AO*, 2; (10) *CO*, 5; (11) *BO*, 3; (12) *CO*, 6.

(2) *Subjects*

Subject W was a third-year psychology student, Subject M was a young officer, and Subject P a second-year medical student. Subjects W and P had studied organic chemistry. All three subjects had been hypnotized once before by one of us (H. B.).

(3) *Procedure*

The experiment comprised three separate sessions held on three successive days. These are called *Waking Session I*, *Hypnotic Session*, and *Waking Session II* respectively.

Waking Session I. The subject was first told that we were interested in the affinity between susceptibility to hypnosis and 'suggestibility' in everyday life. He was not encouraged to suspect any direct connexion between the two waking sessions and the hypnotic session. The series of cards was always given first, and the odours immediately afterwards. Presentation of the latter was not rigidly standardized. The subject was merely told to close his eyes and was given the following instructions: "I'm going to give you three odours. I want you to tell me anything that the smells suggest to you. Tell me anything that comes into your mind when you smell each one." The stopper of the first bottle was then removed and the subject allowed to sample the odour to his own satisfaction. Any comments made were recorded. After 2 or 3 min. the next odour was given in the same way. Likewise for the third odour.

The three stimuli were then given again, in the same order, but in each case the subject was told the *name* of the odour, and asked to try and associate the two.

Hypnotic Session. The subject was hypnotized by one of us (H. B.), the other keeping a careful record of the procedure. Details of the general procedure have already been given (1). The three critical odours were given in succession shortly after the last member of the picture series. The instructions were as follows: "I have here three different odours. I want you to smell each one in turn and try to tell me what it is. you don't know the name, tell me anything that the odour suggests to you." All responses were recorded in full. The odours were then presented a second time, but in this case the subject was also told the *name* of each odour. On removal of the first odour, the following suggestion was given: "After you wake up, you will have forgotten all about having smelt this odour, and if you should smell it to-morrow you will be unable to say what it is." Exactly the same suggestion was repeated on removal of the second and the third odour. When all the material had been given, a general suggestion was made to cover both odours and cards: "After you wake up, you will have forgotten all about the cards and also all about the odours. You will forget all about them. You will not be able to recognize the cards. You will have forgotten all about the cards and all about the odours, and you will not be able to remember that I presented them to you." The subject was awakened shortly after this suggestion had been given.

Waking Session II. This took place about 24 hr. after the hypnotic session. The subject was told that it was a continuation of the work begun on the first day.

The twelve odours (*AO*+*BO*+*CO*) were given before the cards, with the following instructions: "I'm going to give you a series of twelve odours, one by one. I want you to smell each one and give it a name if you can. But be sure and tell me everything that comes into your mind in connexion with each odour. Tell me any names, or past scenes, that the odour may call up, whether you have smelt it before or not, whether it reminds you of anything or not, or in general how you would describe it." The odours were then given in the order stated in § (1). No time limit was imposed, but the time elapsing between presentation of the stimulus and the first verbal response was

always recorded. In some cases, additional presentations of the stimuli were given at the end of the series. An interval of 2 to 4 min. was allowed between each stimulus. Responses were taken down *verbatim*.

In the case of one subject (M), each session was conducted by a different experimenter. Dr J. M. Blackburn, who kindly presented all the material for recognition on the third day, had no previous information about the construction of the test series.

III. Results

(1) *Subject W*

The responses given to Series *AO* during Waking Session I will be considered first; then those given to Series *BO* during the Hypnotic Session; and lastly, the reactions given to the three sets, *AO*, *BO*, and *CO*, during Waking Session II.

Waking Session I. Series AO. One of the three odours was correctly named. One was described carefully, and a possible name suggested. One gave rise to a definite paramnesia, in the sense in which this term has been defined in our earlier paper. It was not of course conditioned by our hypnotic procedure.

AO, 1: *Nitrobenzene.* "I should say nitrobenzene. Partly because it's slightly different from prussic acid, and partly because I didn't think you'd use prussic acid!"

AO, 2: *Valerian.* "Can't get it. An oil of some sort. Smells rather heavy and sweet. I seem to have smelt it before but can't trace it down to anything."

AO, 3: *Aniseed.* "Smells a little like some of the things they mix for cough mixtures—don't know what it is. It's something dissolved in alcohol. Is it chlorodyne?"

The odours were then given again and the subject told their names in the usual way. He accepted all without question.

Hypnotic Session. Series BO. The following responses were given to these three odours:

BO, 1: *Capronic acid.* "Don't know. Something sour....Can't say. Some sort of cheese or something."

BO, 2: *Creosote.* "Don't know what it is. It smells like a chemist's shop...form of antiseptic sort of stuff. Smells spicy, rather."

BO, 3: *Thymol.* "Smells rather like the second, but rather heavier. A little bit like oil of wintergreen—but not quite."

The odours were given a second time together with their respective names, and the post-hypnotic suggestions of amnesia were given as described.

Waking Session II. The responses given during Waking Session II to Series *CO*, *AO*, and *BO* will be considered separately.

Series CO. One of the six odours was recognized and given its correct name. Three were described in detail and possible names tentatively suggested in each case. This naming was of a trial-and-error character, obviously based on analogy. One odour was correctly treated as a member of the general class to which it belonged and aroused some recognition. One odour elicited a distinct paramnesia—in the sense of an impression of familiarity with inability to recall the required name.

We may give some illustrations of these responses:

(8) *CO*, 4: *Camphor.* "Camphor, isn't it?"

(3) *CO*, 2: *Juniper.* "Smells like pure resin—generally tarry, resinous. Pine idea —like a pine shampoo. Anthracene?"

(12) *CO*, 6: *Verbena.* "A citronish sort of smell—probably citric acid. Very lemon smell. Smells rather sweet."

(10) *CO*, 5: *Perfume.* "A common or garden scent of some kind. I don't remember the name. I think it must be fairly popular. I've smelt it quite a lot before."

(1) *CO*, 1: *Vanilla.* "Can't trace it. I can't get the name. It's the smell of a certain kind of chocolate—an essence of something."

Series AO. One of the three odours in this series, nitrobenzene, was correctly named and related to the actual session on which it had been given. Recognition was rapid and complete. One of the odours was said to be "like aniseed" when it in fact was aniseed. It was not said to have been given on an earlier session. Recognition was thus incomplete, since reference was not specifically made to the earlier presentation. The third odour gave rise to a response with obvious paramnesic elements. The subject thought it unlikely that he had been given this stimulus on the first day of the experiment. These three responses are given in full for purposes of comparison with those to the odours given in the Hypnotic Session:

(2) *AO*, 3: *Aniseed.* "Rather like aniseed. Smells like some form of medicine—a spicy sort of smell."

(7) *AO*, 1: *Nitrobenzene.* "Nitrobenzene: the first on Monday (the first day of the experiment). It always reminds me of furniture polish of a certain sort."

(9) *AO*, 2: *Valerian.* "One I've smelt recently. No, I don't think on Monday—it may have been. I can't name it."

These three odours were given again at the end of the whole series and the subject was told their respective names. He then recalled that the aniseed had been given on the first day. He also recalled the name 'valerian', but had failed to associate it with the requisite stimulus.

Series BO. The three odours in this series gave rise to responses of a distinctly paramnesic type. The paramnesia was quite immediate in two cases, but rather less so in the third.

(4) *BO*, 1: *Capronic acid.* "I think I've smelt that before sometime. Smells like something very sour—sour cheese." *Can you describe your experience on smelling that one?* "I probably smelt it in the lab. sometime—yet I don't know. It wasn't one that I smelt on Monday. It seems to have stuck in my mind. Perhaps I smelt it in the lab. a year or two ago."

The above response shows all the characteristics of experimental paramnesia. First, there is the usual judgement of past reference; secondly, an effort to recall the earlier circumstances; thirdly, a suggestion (rationalization) that the odour may have been last met with in the laboratory a year or two earlier.

(6) *BO*, 2: *Creosote.* "A heavy sort of smell—rather tar-like. Smells like that come from soap. I don't remember having smelt it—not quite the same before." *Do you think you've smelt something like it before?* "Something like it (affirmatively), but something more volatile and rather overpowering." *Do you know what it was or where you smelt it?* "I think it was one of the coal-tar things. I seem to remember smelling it in a chemical works—one of the light products."

These answers suggest, first, that the subject recalled not the same but a similar odour, and secondly that he associated the latter with a chemical works. It appeared later on, however, that this reduplicative explanation was probably rationalization. When the subject was giving us some general retrospections at the end of the test, he remarked spontaneously that "the pine-resin is the one that sticks most—it seems very familiar". Now he may have meant by the "pine-resin" either *CO*, 2 (juniper) or *BO*, 2 (creosote). He was, therefore, given both these odours again and asked which he meant. His replies were as follows:

(3) *CO*, 2: *Juniper.* "I think that's the lighter one." *Is that the one that sticks?* "No, that's the other one."

(6) *BO*, 2: *Creosote.* (Emphatically) "*That's* the one! I *know* I've smelt it before somewhere. I'm almost certain I have. It's the sort of smell one doesn't forget. It may have been one of those sprays that people spray—as for influenza. Or it may be one of those things used in hair shampoos. I rather think I've smelt it *as itself.*"

These responses are interesting in that previous acquaintance with the creosote was at first denied but later affirmed with considerable vigour. The subject appeared unable at the end to recall any situation which would satisfactorily explain the familiarity set up by this stimulus. No fewer than three tentative explanations were given—all of which were rejected as inadequate.

(11) *BO*, 3: *Thymol.* "I've smelt that before. Rather ethereal—fruity." *Can you give any description of your experience on first smelling that one?* "I seem to have a

vague idea. Oil of wintergreen seems somehow connected with it....Not a sort of smell that one would be likely to forget." *You do think that you have smelt it before?* "Yes: I think I've compared it with oil of wintergreen before. I think I've confused the two before sometime."

Here we have an interesting illustration of recall without recognition. The comparison of the thymol with oil of wintergreen was of course made on the previous day during hypnosis. The subject could apparently recall having made this comparison, but was unable to formulate the conditions under which it was effected. The statement that the stimulus was "not a sort of smell that one would be likely to forget" is also significant. It probably represents a rather special mode of rationalizing the experience and evading the hypnotic prohibition. One might suggest that the dissociation in this case was not absolutely complete.

We asked the subject (who was a psychology student) whether he could define in general his experience on sampling an odour which he could neither name nor 'place' in his previous experience. He replied that "it was more a case of 'feeling' that I'd smelt a thing before—not thinking it out. The thinking came afterwards—as to the question 'Where?'" This confession lends support to the view that the act of reference to the past was based on an affective reaction.

We may give a brief summary of the results obtained with Subject W on the tests of olfactory recognition:

(1) One of the six control, *CO*, odours was named correctly; three were described in detail and possible names suggested in each case. One odour was ascribed to the general class to which it belonged. This is being counted as a case of normal recognition, for purposes of scoring, despite the fact that the actual trade-name was not given. One odour gave rise to an 'impression of familiarity' with inability to recall the desired name. This we have classed as a paramnesia.

(2) One of the three *AO* odours was correctly identified and referred to the session in which it had been given. One was named, but was not related to the earlier session. One gave rise to what we have called the paramnesic type of response.

(3) All three *BO* odours provoked distinct paramnesias. These responses involved, as with the visual material, (*a*) an act of reference to the past, (*b*) an attempt to define the conditions of previous acquaintance, and (*c*) an element of rationalization, sometimes pronounced.

(4) Five of the twelve responses betrayed some element of paramnesia. Three of these were given to the three odours presented during the hypnotic session.

(2) *Subject M*

We shall consider in order the responses given on the three successive sessions of the experiment. The responses made on the third day during Waking Session II are given in full.

Waking Session I: Series AO. This subject responded in a very full and discursive manner to all three stimuli. We quote extracts from his accounts of each odour:

AO, 1: *Nitrobenzene.* "Burnt almonds. Reminded me of all the ways of eating almonds, particularly of eating them in Germany....Also of war gas...."

AO, 2: *Valerian.* "Reminded me of an operation on my foot. Heat of the room and oil put on my foot. I remember the nurse behind me telling jokes and the surgeon being irritated...."

AO, 3: *Aniseed.* "I think it is used by doctors to make medicine palatable to children....Doctor arrived by car to treat me for scarlet fever. Eaten sweets called gob-stoppers...lasted through long cinema performances...."

There is nothing of note in these responses other than their prolixity. Certain of the reminiscences, however, appeared again during the recognition test on the third day.

Hypnotic Session: Series BO. The responses made by this subject under hypnosis are given below. It will be recalled that hypnosis was moderately deep.

BO, 1: *Capronic acid.* This stimulus called forth a dramatic reaction. The subject uttered in a very solemn tone of voice the phrase: "Just *Leichengift.*" When asked to explain what he meant, he became very agitated and started to talk rapidly in a low voice. We were able to detect the words: "*Leichengift*—it is the smell of dead soldiers." He then described an experience which he had undergone during which he had been in contact with corpses in a state of putrefaction. We believe that this experience was genuine, though we have no definite evidence on the point.

BO, 2: *Creosote.* "Reminds me of the operating theatre and all the volatile odours one has there: heat, steam, water-bandages...light from the globe; noise from the sterilizing apparatus...."

BO, 3: *Thymol.* "I couldn't tell you what it is but I'm sure we use it when we have sprains...when you have a strained wrist—from running too hard. It may be a burn dressing too."

None of these three odours was identified by name, though all seemed to possess a striking redintegrative value for this subject. The reaction to the first was especially interesting, and, as will be seen, had repercussions on the recognition test the following day. As usual, the three odours were given a second time during hypnosis and the subject was told their names.

Waking Session II: Series CO. One of the six new stimuli was named

correctly. Three could not be named but gave rise to redintegrations of the general type described. One was confused with an odour given during the first session, and one gave rise to a definite paramnesia. Some extracts from our protocols will serve to illustrate these different types of reaction:

(8) *CO*, 4: *Camphor.* "I am sure I have smelt that before. It is camphor. It is something that played a large part in the first years of growing up...."

(1) *CO*, 1: *Vanilla.* "That reminds me of a certain sweet we used to eat as children....I can remember the colour of the sweets. They were pink or blue and smooth as an egg...."

The following odour was confused with *AO*, 3 (aniseed). This was probably due to the very faded preparation of civet which we used and not to defective discrimination on the part of the subject.

(5) *CO*, 3: *Civet.* "Yes, I'm afraid I haven't got a good nose for things, especially if I have to smell one thing after another. But I am sure it formed part of the sweets we used to eat. I think we had that on Monday. I think I told your colleague it was a thing called a 'gob-stopper'."

The last odour in the series gave rise to an indubitable paramnesia of recognition. It must be borne in mind, however, that this odour was given directly after *BO*, 3, thymol, which induced one of the most striking paramnesias in the whole series. There may therefore have been some contamination of this stimulus by its predecessor.

(12) *CO*, 6: *Verbena.* "Dear me! What a poor memory I must have. This is most irritating. I am sure I have smelt that before. I wonder if I smelt that on Monday? (the first session).—I don't know—I am sure I know something connected with it. My mind seems to be very blank. I am sorry, I can't tell you. The trouble is I can't really say whether I smelt it on Monday now. No, I can't fix that."

This response betrays all the features which we have decided to treat as paramnesic. But we are not aware that the subject had experienced the odour before, though it is certainly fairly common. It was not especially similar to any of the *BO* odours given during hypnosis.

Series AO. None of the three stimuli in this group was identified in the ordinary sense of the term. It therefore follows that the reactions to the *BO* odours are less well controlled than those to the corresponding pictures discussed in the previous paper. But a distinctly paramnesic reaction was given to only one of the three *AO* odours, whereas all the *BO* odours elicited responses showing a more marked element of paramnesia. It is convenient to describe the two non-paramnesic responses here, leaving the third to be considered along with the responses to the *BO* odours.

(2) *AO*, 3: *Aniseed.* "I am almost certain that is the component of medicine that again I had as a child in sore throats, etc. I remember the house we were in then. It is also part of some sweet, I think...."

There was no recognition of the aniseed in the sense of naming or reference to the previous session. But the responses called out on the two occasions were extremely similar in content. This is perhaps another example of recall without recognition.

(7) *AO*, 1: *Nitrobenzene.* "I can't remember much about that. It reminds me of one of the innumerable things you smell in a chemist's shop, making up one of the pleasant smells you smell in a chemist's, My mind seems pretty blank on that. I can't remember much."

In this case also recognition was completely lacking. Indeed, the associative responses called up are quite different from those given during Waking Session I. It is a clear-cut recognition failure.

Series BO. The first stimulus, capronic acid, gave a very interesting reaction. It will be remembered that this stimulus had excited the subject considerably when given during hypnosis. The response on the recognition test was hardly less dramatic.

(4) *BO*, 1. *Capronic acid.* "I am sure I have smelt that somewhere before. It is a very unpleasant smell. It has something to do with decayed vegetation or something like that in a forest. I am sure that sometime to-day I shall suddenly remember something very unpleasant connected with it. I am sure I have nearly remembered where I smelt that. Let me see....It is on the tip of my tongue to tell you where I smelt it, but I....No, I just can't tell you. Probably I shall know later on. It seems as if I almost knew where I had smelt it before and yet I can't formulate it in words. Yes! I know where I....Yes, that's it—something to do with the dead. Yes I know now. I am most awfully sorry I don't want to tell you. Shall we leave it? It was a most unpleasant experience." (*Note by J. M. B.*: The subject looks very pale, and after a pause I say: "Right. If you can't, we will leave it.")

It is probable that the inhibition induced by the hypnotic suggestion on the previous day was here reinforcing a pre-existing repression. Certainly the events described during hypnosis in response to this odour were exceedingly unpleasant, and it is not surprising that they should have undergone repression. But we cannot say for certain whether the acute paramnesia was the result of this repression or whether it depended primarily on the hypnotic inhibition. On the one hand, the paramnesia was dispersed on recall of the associative context. On the other hand, the reaction was extraordinarily similar to those given in cases in which there was no such additional repression. Probably the paramnesia depended on both these factors, and was the outcome of the two-fold resistance to recognition.

The reaction to capronic acid is interesting for a number of reasons. First, there was the immediate act of reference to the past so typical of this general class of reactions. Secondly, there was an abortive rationalization: the odour was associated with decayed vegetation. Some may see in this a symbolic euphemism, but we shall not press the point. Thirdly, the subject gave a beautiful account of a reaction just below the threshold: he reported that something unpleasant was associated with the odour which he thought he would recall later in the day, and then that the earlier setting was on the "tip of his tongue" but could not be put into words. This brings out in an interesting way the connexion between paramnesias of recognition and inhibited recall. Fourthly, the setting of the original service experience was suddenly recalled and formulated. But the description given was very oblique and bowdlerized. No reference was made, for example, to *Leichengift* or to the corpses of soldiers. Finally, we may reiterate that no suggestion was made at any point that the capronic acid had anything whatsoever to do with the events of hypnosis.

We may insert a brief comment here on the response to the third odour of the *AO* series given later during Waking Session II. The odour in question was valerian, *AO*, 2. The subject entirely failed to recognize this stimulus, and his response had clear-cut paramnesic features:

(9) *AO*, 2: *Valerian.* "I am sure it was that unpleasant smell again (i.e. capronic acid)." (*Note by J. M. B.*: Silence for some time. I assure him it was not.) "Why should it remind me of that? I can't tell you anything except that it just reminds me of that."

The above laconic response contrasts markedly with the circumstantial accounts generally given by this subject. It is possible that the smell of valerian resembled in some degree that of capronic acid. It is quite certain that the subject interpreted it as doing so. We have a clear example of a repression, set up in connexion with an odour given under hypnosis, determining the response to an odour previously given in the waking state. A somewhat similar type of interference was described in some of our earlier results with visual material.

The second odour of the *BO* series was creosote. The subject's response to this stimulus was as follows:

(6) *BO*, 2: *Creosote.* "I am sure I have smelt that in the operating theatre. I can remember such a smell like that connected with the apprehension with which one awaits even the slightest operation. The sound of instruments being collected together, and the sound of water bubbling out of the taps....I can almost feel the needle penetrate."

This behaviour shows redintegration without recognition of the

stimulus itself, nor of the fact that these associations had been given in almost the same words on the previous day. We cannot call this a case of paramnesia. Rather must it be classed as an amnesia—as an *absolute* recognition failure for hypnotic material.

The third odour in this series was thymol. In this case, the response was markedly paramnesic, and it also throws some light on the mechanism of hypnotic inhibition.

(11) *BO*, 3. *Thymol.* "I wonder where I have smelt that before? Perhaps I smelt it when we were in the workshop down below before coming up here. And I have a feeling that as soon as I get home I shall remember something else about it too. No, I'm sorry, I am surprisingly (dense). I feel I have got something to tell you about it too, but I can't get it fixed. I can see a word in my mind and I just can't say it. It is—er—I keep asking myself why I don't hurry up, and say it, but (*laughs*)—the word is 'yesterday'—(*very loudly and distinctly*)—That was quite an achievement, I'm sure!" *What was?* "Saying that. I saw the word as clearly as I see that graph over there and I saw it in my mind." *Why couldn't you say it?* "I couldn't bring it into any sentence....I feel exactly like a small boy who keeps on raising his hand to say something and the teacher never looks at him."

This reaction, besides having all the characteristics of local paramnesia, is marked by the intrusion into consciousness of a presumably dissociated imaginal response. At any rate, this image must have been the outcome of the unconscious recognition process. The reaction as a whole shows the following stages: (1) The subject at once asked himself where he could have smelt the odour before, thus betraying an act of reference to past experience. (2) He first answered his own question by reference to the odour of the workshop which he passed on his way to the room in which the experiment was being conducted. This was probably construction of a rather facile character, for no odour of this kind was present in the workshop itself. (3) The subject then described a certain difficulty in recalling data which he believed to be connected with the thymol. (4) There was an intrusion into consciousness of the word 'yesterday' carried in visual form. (5) There was considerable difficulty in communicating this word to the experimenter, and it is interesting that the subject found it difficult to include the word in any sentence. (6) The subject appeared fairly satisfied with his effort of recall. On the other hand, he did not betray any awareness of the significance of the word produced with so much labour. He did not go on to suggest any connexion between it and the events of the previous day. It is possible that he was in some degree aware of the connexion but he was entirely unable to state it.

A short summary of the results obtained from Subject M on the third day is given below:

(1) One new stimulus was named correctly and three were named by analogy with more familiar odours. One stimulus was tentatively ascribed to the Waking Session I, and one gave rise to responses of the paramnesic type.

(2) Three of the six *CO* stimuli aroused reactions of a redintegrative character. Some of these went back to situations of childhood and early life.

(3) None of the *AO* odours was identified. One of the three was regarded as completely new. The second called out responses similar to those given at the earlier session, but without recognition. The third was associated with an odour given during hypnosis which had elicited a marked paramnesia.

(4) Two reactions to *BO* stimuli were strongly paramnesic. In the first, there was good evidence that the hypnotic inhibition was operating in conjunction with a pre-existing repression. In the second, a visual image suddenly appeared in consciousness which carried some reference to the hypnotic session. But the subject certainly lacked insight into the significance of this image.

(5) The third *BO* stimulus aroused redintegrative reactions almost identical with those given the previous day under hypnosis. There was no trace of recognition in this case.

(6) A certain similarity between the results of our hypnotic procedure and normal paramnesias are indicated though our control with the odours is obviously less exact than it was in the case of the pictures.

(3) *Subject P*

The results obtained from this subject are presented in the usual way.

Waking Session I: Series AO.

AO, 1: *Nitrobenzene.* "Immediately thought of the chemistry labs. Some aromatic organic substance—not aniline. I was immediately transported to the chemistry labs."

AO, 2: *Valerian.* "Reminds me partly of a chemist's shop, and before that of moth-balls and a fur coat...."

AO, 3: *Aniseed.* "Once again I thought of the chemistry labs. Also reminded me of oil of wintergreen—and tooth paste. The sort of smell that hangs around on tooth glasses and the wash basin."

The three odours were given a second time in the usual way and the subject informed of their identity.

Hypnotic Session: Series BO.

BO, 1: *Capronic acid.* "Smell of vinegar." *Is that all you have to say about it?* "Yes."

BO, 2: *Creosote.* "Tar of some sort. Reminds me of asphalt."

BO, 3: *Thymol.* "Reminds me of some leaf or herb. Can't place it at all."

Again the name of each stimulus was told to the subject on the second presentation of the series.

Waking Session II: Series CO. None of the six odours was identified, though several were named by analogy or by reference to their probable sources in common experience. We may quote a few of the reactions to these odours:

(1) *CO*, 1: *Vanilla.* "Reminds me of chocolates. It's something used in confectionery and cakes."

(3) *CO*, 2: *Juniper.* "Some aromatic oil. Creosote or something used for varnishing woods."

(8) *CO*, 4: *Camphor.* "Reminds me of 'Vix'—one of those inhalents one uses for 'flu."

No case of paramnesia, in the sense of unspecified reference to the past and its accompanying perplexity, was observed in this series.

Series AO. None of this material was identified, but two stimuli evoked paramnesias. The individual responses were as follows:

(2) *AO*, 3: *Aniseed.* "Not liquorice. Used in cough mixture."

(7) *AO*, 1: *Nitrobenzene.* "I can't place that smell at all." *Do you think you've smelt that before?* "Yes. I can't place it though."

(9) *AO*, 2: *Valerian.* Can't place that either. I have smelt that before."

When all the recognition material had been given, the subject was asked whether he had recognized any of the odours given on the first day. He replied: "Well, we didn't have nitrobenzene, but I thought the eleventh (*BO*, 3) (thymol) might be the valerian. One or two were vaguely familiar, but they may have been in the 'organic' labs or somewhere." He also asked us whether the last odour given during the first session was aniseed and we told him that this was the case. The three *AO* odours were then given again as a further test of normal recognition.

(7) *AO*, 1: *Nitrobenzene.* "I remember that. That isn't aniseed is it?" *Can you describe your experience on smelling that?* "A tarry smell." *You feel it to be familiar?* "Yes, surely." *That's the nitrobenzene.* "Oh, that's it! I knew I'd had that before."

(2) *AO*, 3: *Aniseed.* "The cough mixture. Can't be sure I've had that one." *That is the aniseed.*

(9) *AO*, 2: *Valerian.* "I can't place that at all. A resinous sort of smell." *That is the valerian.*

One may note that two of the three original *AO* odours elicit some

degree of paramnesia. In these cases, the difficulty in learning the names of discrete odours seems a sufficient explanation of the responses. The *BO* material, however, gave rise to greater perplexity and the responses suggested that some degree of recognition was present.

Series BO. The first stimulus was capronic acid. It will be remembered that the subject associated this odour with vinegar when it was given under hypnosis.

(4) *BO*, 1: *Capronic acid.* "(Marked hesitation) It reminds me of vinegar. I don't know what it is called."

(6) *BO*, 2: *Creosote.* "Tar. Oil."

(11) *BO*, 3: *Thymol.* "Some oil or resin. Not oil of cloves but something similar. Would that be the valerian?"

When the subject was asked at the end of the session to specify the few odours which he had himself thought to be vaguely familiar, he made special reference to *BO*, 3. This odour was therefore given a second time.

(11) *BO*, 3: *Thymol.* "No, I don't....It's familiar all right but I can't name it." *Can you describe to me your experience on smelling that one?* "Well I know I've smelt it recently. It's a sufficiently unusual smell that one wouldn't have come across often."

The other two *BO* stimuli were also given a second time and evoked the following answers:

(4) *BO*, 1: *Capronic acid.* "That's the vinegar smell. That, too, smells vaguely familiar, but I was meddling with vinegar the other day. It's vaguely acidic. It's not the one I suggested a little while ago as the valerian."

(6) *BO*, 2: *Creosote.* "That's the tar one. It's faintly familiar....I can't remember the name."

It is difficult to draw definite conclusions from this group of paramnesias. As we have seen, two members of the *AO* series called forth responses of the same general type as those given to the *BO* stimuli. If anything, the perplexity engendered by the latter was slightly more marked, and the rationalization of *BO*, 1 in the sense of "meddling with vinegar" was quite in line with the general character of our experimental paramnesias. But any conclusions drawn from this material alone must be treated with particular caution.

A brief summary of the results from Subject P may now be appended.

(1) Five cases of paramnesia were recorded. Two of these were to odours given during Waking Session I.

(2) There were no gross differences between the paramnesias evoked by the *AO* and the *BO* series.

(3) The results obtained with the *BO* stimuli were in line with those of the other two subjects. The subject did not suggest that any of the

odours might have been given under hypnosis. In one case, he rationalized in an interesting manner.

IV. Quantitative data

Table 1 shows the distribution of the whole of the responses made to the various stimuli on the third day. In col. 1 are given all instances in which the material was definitely recognized. These include (*a*) cases in which a stimulus was referred to the actual session on which it had been given, and (*b*) cases in which a stimulus was correctly named. Col. 2 shows the number of times a stimulus was displaced from one session to another. There is only one entry here—a *CO* stimulus was wrongly attributed to the first session. In col. 3 are entered all cases in which a stimulus was treated as new, i.e. the subject stated or implied that he had not smelt it during either of the previous sessions. This group corresponds to our category of 'rejections' on the card-recognition test. In col. 4 all responses marked by an element of paramnesia, both 'experimental' and uncontrolled, are included.

Table 1

	Col. 1 Recognition			Col. 2 Interserial displacement			Col. 3 Rejections			Col. 4 Paramnesias		
Subject	*AO*	*BO*	*CO*	*AO*	*BO*	*CO*	*AO*	*BO*	*CO*	*AO*	*BO*	*CO*
W	2	—	2	—	—	—	—	—	3	1	3	1
M	—	—	1	—	—	1	2	1	3	1	2	1
P	—	—	—	—	—	—	1	—	6	2	3	—
Total	2	—	3	—	—	1	3	1	12	4	8	2

The quantitative results are much less clear-cut than those given in our earlier paper. Two subjects, M and P, entirely failed to recognize the odours given during Waking Session I, and the third (W) was able to identify only two of these three odours. We have to record three cases of absolute, and four of relative, recognition failure with the *AO* odours. These cases of relative failure are essentially paramnesic, and are in no important way different from the paramnesias induced by the hypnotic technique. On the other hand, it is important to recognize that eight out of nine responses to the *BO* stimuli are of the paramnesic type. This means that 90% of the responses to these stimuli were paramnesic whereas only 22% of the responses given to the *AO* and *CO* stimuli were of this type. For the *AO* odours alone, i.e. those given during Waking Session I, the corresponding figure is 44%. There is thus some evidence, slight it is true, that the hypnotic procedure favoured the appearance of paramnesia on a test of olfactory recognition.

The reaction times of the responses during Waking Session II were recorded. They do not show any significant differences in the means of the reaction times to the *BO* odours as compared with those to the *AO* and *CO* series. For W and P the mean reaction times in cases of paramnesia were slightly greater than the mean reaction times in all non-paramnesic responses, but the differences were not significant. Subject P, on the other hand, had a slightly shorter mean reaction time for the responses which we have classed as paramnesic. In all cases the scatter was very large. No definite conclusions can be drawn concerning the relations between the temporal and qualitative aspects of these paramnesias.

V. Discussion

We must first consider what influence, if any, our hypnotic procedure has upon the emergence of paramnesia. It has already been mentioned that 44 % of the stimuli given during Waking Session I and 90 % of the stimuli given during hypnosis produced paramnesic responses on the third day. Considering, further, the proportion of paramnesic responses produced by all the odours on the third day, the following figures are obtained: (*a*) 57 % of the total recorded paramnesias were caused by *BO* odours; (*b*) 29 % were evoked by *AO* odours; and (*c*) 14 % by *CO* odours. It thus seems fair to conclude that presentation of an odour which has been associated with post-hypnotic amnesia is more likely to elicit paramnesia than presentation of an odour previously given in the waking state.

This result, though striking, is not so decisive as those obtained with the picture material. In our earlier paper, no case of paramnesia due to pictures seen during either of the waking sessions was recorded, save where a picture bore some considerable degree of similarity to one shown during the hypnotic session. In our present experiment, on the other hand, no less than 44 % of the stimuli presented during Waking Session I gave rise to paramnesias of varying degrees. It is obvious that our control was inadequate.

There are no outstanding qualitative differences between the paramnesic responses to visual and olfactory stimuli. In all cases, the first moment was always an act of reference to the past. The stimulus was given meaning in terms of personal experience. This was rapidly superseded by an effort to recall the original setting of the stimulus. Owing to our special conditions, this effort of recall often gave rise to constructive rationalization. In some cases, however, a subject might betray knowledge of further facts associated with a stimulus, but would find himself

unable to formulate these facts. Subject M, for example, showed considerable insight into his perplexity. But in general, the nature and course of experimental paramnesia was the same with both visual and olfactory stimuli, and it is probable that it would be found to be the same whatever the type of stimuli with reference to which it was set up.

We have recorded two somewhat atypical olfactory paramnesias which merit a short discussion. These were obtained from Subject M in responding to two of the odours given under hypnosis. It will be remembered that this subject associated one of these odours, namely capronic acid, with a traumatic episode in his military experience. There was also evidence that this episode had been in part at least repressed. On the third day, capronic acid gave rise to an outstanding paramnesia which the subject at first found himself totally unable to interpret. Eventually he succeeded in relating the odour to the experience in question and the paramnesia was apparently dispersed. But there appeared to be no connexion in the subject's mind between the smell of capronic acid and the experiences of hypnosis. It would seem that we have in this case two repressive factors, arising at different times and for different reasons and reinforcing one another in producing a certain type of response. Situations of this kind raise interesting problems connected with the study of repression.

The second atypical paramnesia occurred when Subject M reported the appearance in consciousness of the word 'yesterday' on being presented with thymol. It is interesting that the subject experienced great difficulty in telling the examiner what the word was and in introducing it into a sentence. Even when he succeeded in communicating the word, its true significance was apparently lost upon him. At any rate, he made no explicit reference to the events of the preceding day. This would seem to indicate that the *meaning* of the word, in so far as it bore on the problem confronting the subject, remained dissociated. More concretely, the subject lacked insight into the relation of his imaginal response to the precipitating stimulus. It follows that the mere appearance of a relevant image in consciousness need not be sufficient to provoke recognition. This result may be of interest to those who regard recognition as dependent on the revival of discrete images. At any rate, we have a fairly clear example of the way in which a subject may lack insight into the significance of an image even when this image can be subjected to introspective scrutiny.

We may pass on to a short discussion of affect in paramnesia. It may be recalled that MacCurdy, in his general discussion of *déjà-vu* (6), holds

that experiences of this character are invariably associated with an unpleasant affect. The affect arises, he believes, in two ways, (*a*) from the failure to resuscitate the details and setting of the earlier experience, and (*b*) from the compulsive character of the impulse to try to do so. We certainly noted that the phase of perplexity in experimental paramnesia might be extremely disconcerting, and in some cases markedly unpleasant. There was but one case, however, in which the reaction could be said to be definitely emotional (Subject M, to *BO*, 1). And in this case the unpleasant affect was undoubtedly determined by an underlying complex. In the remaining cases, the dominant affect was one of perplexity, mildly irritating, and doubtless arising from MacCurdy's two sources.

It is obvious that our experimental paramnesias occurred under conditions in which recognition was impeded by a process akin to repression. But we do not wish to imply, nor does it seem justifiable to assume, that repression must always precede the appearance of paramnesia. We have accumulated a fair amount of evidence (not yet published) that this phenomenon may be associated in a large number of cases with the perception of objects in unwonted or unexpected settings. As an example of an olfactory paramnesia arising in this way, we may quote a report kindly furnished us by a Cambridge anthropologist (Dr E. J. Lindgren). The similarity of her experience to the phenomena provoked under our experimental conditions is self-evident:

> I was recently driving Mr and Mrs L. from my house into Cambridge when I became gradually aware of the most delightful sensation, and presently identified it as caused by a smell. This seemed so exceptionally overwhelming in its pleasantly drug-like effect that I felt obliged to attend further to it and to try to give it a name. Suddenly it came: 'Mongolia.' There was just time for me to think: "But L. cannot *smell* of Mongolia when he left it nine months ago", when I exclaimed: "Oh! What *have* you got with you?", and turned round to find him smoking a pipe and hear him explain that it was in fact Mongol tobacco.

The altered setting of the stimulus in the above example is probably sufficient to account for the striking paramnesia. It is obvious that the odour in question would have been recognized immediately had it formed part of its accustomed setting. Indeed in this case, as in many similar cases which we have recorded (though not yet published), the primary obstacle to recognition is bound up with the necessity for a certain reorientation. There is difficulty in changing from the prevailing orientation to the one demanded by the situation in which the intruding element is normally to be found. In such cases, the incongruous element does not provide a sufficient stimulus for the readjustment demanded. The resulting paramnesia expresses both the need for a reorientation and the

difficulty in bringing it about. It is at the same time a challenge and a confession of failure.

We cannot enter here into a full discussion of orientation and its significance in recognizing. But we should like to emphasize the usefulness of the general idea and the part which it has played in the theory of memory. Betz(3), the first to think along these lines, founded his notion of *Einstellung* on an analysis of paramnesia. Claparède, in an unduly neglected contribution(4), has given an analysis of paramnesia very much along the lines of our present work. In more recent times, Bartlett(2) has laid particular emphasis on the orientation factor and its role in recognition. MacCurdy(7), and after him Koffka(5), devote some discussion to the same problem. In general, we think it correct to state that responses of the paramnesic type are forthcoming wherever a perceptual pattern fails to call out the orientation appropriate to it. This may result from a number of causes. In the example given above there was a genuine conflict with the prevailing orientation. In our experiments we have to deal rather with an artificial check imposed by the post-hypnotic inhibition. But in all cases paramnesia expresses the need for an active readjustment of attitude. Its functional significance is indeed obvious.

The relation of our earlier results to *déjà-vu* was indicated briefly in our first paper. Since that paper was written, Dr R. W. Pickford has introduced us to some material which suggests that the connexion between the different types of paramnesia may be closer than we had suspected. We hope to deal in a later paper with the relation of our results to Dr Pickford's recent work(8) and to the general theory of memory.

VI. Summary

1. Illustrations are given of restricted paramnesias set up in response to olfactory material in three subjects.

2. The paramnesias were most common with material for which post-hypnotic amnesia had been induced on the previous day.

3. Control of the hypnotic variable was less complete than in the experiments with visual material. The results nevertheless indicate a definite association between the inhibitions given under hypnosis and the appearance of paramnesia during the recognition test.

4. The paramnesias resembled in their general character those set up in response to visual material under identical experimental conditions.

5. Two atypical paramnesias are described which illustrate certain aspects of repression.

6. Affect in paramnesia is briefly discussed.

7. The relation of paramnesia to the orientation factor in recognizing is considered. The significance of this factor for effective reaction is emphasized.

Our thanks are due, and are tendered, to all those who helped us in these experiments, in particular to Drs J. M. Blackburn and K. J. W. Craik; and to Dr R. W. Pickford for some very interesting suggestions to which we hope to do full justice in a subsequent paper.

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(*Manuscript received* 10 *April* 1941)

PUBLICATIONS RECENTLY RECEIVED

Organizing and Memorizing. By GEORGE KATONA. New York: Columbia University Press (London: Humphrey Milford, Oxford University Press). 1940. Pp. xii+318. 23*s.* 6*d.* net.

This book consists mainly of a detailed account of a most ingenious series of experiments designed to show the much greater speed and efficiency of learning by 'understanding' of a situation, in this case the apprehension of the essential features of a series of problems, than by rote memorizing of the solutions of such problems. Moreover, the understanding gained by the first method of learning can be transferred effectively to other similar situations, whereas memorized solutions give little if any assistance in dealing with other similar problems. Although the problems investigated are verbal and arithmetical, the author shows that understanding which is adequate for solving the problems can take place without the necessity of fully formulating the method of solution. This is an important conclusion, since it indicates the possibility of a transfer, through such an understanding of the situation, in the acquisition of skills other than verbal. The author does not deal with such skills, but it would be of the greatest interest and value if he could extend to their study his admirable technique and experimental design.

On the theoretical side, he tries to link up his results with Koffka's trace theory of remembering. This part of the argument is less convincing. But the important point is made that there are not two fundamentally different learning processes, (*a*) the learning of meaningful material, (*b*) the memorizing of meaningless. But there are differing degrees of organizing, with a maximum amount in learning through 'understanding', a minimum amount in rote memorizing; no learning occurs unless there is some organizing, e.g. in rhythmically arranged groups.

This most interesting and valuable study of the learning process is highly recommended to all psychologists.

The Psychology of the Interview. By R. C. OLDFIELD. London: Methuen and Co., Ltd. 1941. Pp. xv+144. 5*s.* net.

This book should perhaps be more accurately entitled 'The Psychological Analysis of the Interview', since this task of analysis it performs most admirably. It was indeed opportune to point out that an interview is not an interchange of stereotyped question and answer, followed by the formation of an equally stereotyped assessment in set terms of the nature of the candidate's personality or fitness for an occupation. Mr Oldfield stresses the fact that the interview is essentially an interplay of attitudes between interviewer and candidate. The former, by a manipulation of his own attitudes (of which he is usually only partly aware), stimulates the latter to react by corresponding changes of attitude. From these the interviewer is able intuitively to formulate his assessment, apparently by creating in his own mind, and then observing and judging, a kind of picture or 'schema' of the candidate's personality as a whole.

This last conclusion is a particularly interesting one, and seems to tally well with Reik's description, in *Surprise and the Psychoanalyst*, of the temporary introjection of the patient's ego into that of the analyst. But Mr Oldfield does not sufficiently discuss the essential difficulty of this process, which is: The 'schema' does indeed take the form of a living 'homunculus', characterized by feelings and action tendencies which cannot be formulated in verbal terms. Thus the interviewer may be able to say to himself, "I know what sort of a man you are", while groping in the dark for words and concepts in which to describe the candidate's personality. Of course if the interviewer's task is merely to choose someone to fill a particular job, he may be able to judge the candidate's suitability intuitively and immediately, and the problem of verbal formulation does not arise. But it is disappointing to the psychologist that it should be left so much in the air; and that Mr Oldfield has been unable to make use of his experimental material to elucidate it further, and to investigate the validity of such verbal character assessments as can be made.

He must, however, be congratulated on his analysis of the interviewing process, and upon the lucid and attractive manner in which he has carried it out.

Human Nature and the Social Order. By E. L. Thorndike. New York: The Macmillan Co. 1940. Pp. xx+1019. 18*s*. net.

This monumental work, of over 1000 pages, gives as exhaustive a review as is possible, viewed from the behaviourist standpoint, of human nature and human institutions. It begins with the behaviour of the individual, his abilities and potentialities, his wants, needs and his ways of satisfying them, and his values. These findings are then applied to the study of social constructions, customs and institutions: social welfare, economics, industry and commerce, finance, politics, law and government. No more thorough treatment of the more superficial psychological aspects of these topics could be possible, and no one approaching them with the behaviourist outlook could go further. Some readers will be interested and instructed, some will find the book unreadable; yet others may lay it down saying rudely, "So what?" But many will, I think, conclude that it marks the limit of the value of psychological atomism, and of the scientific rationalism and humanitarianism which the author advocates, not realizing that the rise of Nazism (which he ignores) has disclosed their bankruptcy. Henceforth, unless there is some attempt to integrate isolated phenomena, to gain some general understanding of them and explain what they 'mean' to the experiencing individual or social group, psychology will soon become as remote from reality as mediaeval scholasticism. M. D. V.

The Nineteen Forty Mental Measurements Yearbook. Edited by Oscar Krisen Buros. Holland Park, New Jersey. 1941. Pp. xxi+674.

Many readers of this *Journal* must have found occasion to consult the *Nineteen-Thirty-Eight Mental Measurements Yearbook*, an enterprise in publication which deserved, and apparently, achieved wide publicity and as wide appreciation. Now comes the Nineteen Forty issue, larger, more ambitious, and definitely better than the earlier volume. The amount of devoted labour which Prof. Buros must have put into this publication must have been prodigious. As should now be very well known, the

general plan is to give a really comprehensive survey of tests and articles and books about tests which have been published during the period under survey. Not only is the material dealt with listed, with authors' and publishers' names and dates and with prices wherever possible, but a very excellent collection of published critical studies of the material is reproduced. The reader is therefore in a very much better position than if he merely had a summary, and nothing else, for deciding whether the tests or discussions dealt with give him what he may be trying to find. No reputable psychological library ought to be without this book, and there can be few teachers of psychology who will not be glad to be able to direct their more intelligent students to its pages. More than that, the worker in almost any applied field of psychology, who is himself searching for tests of practical value, will find here a mine of most valuable information. It is clearly one of the first essentials, in any volume of this kind, that the arrangement of sections should be lucid and systematic and the indexing very good. It is difficult, in this case, to see how these matters could be done better than they are. It is probably inevitable that there should be some omissions, but the present reviewer who has reasonably good opportunities for becoming aquainted with current work in at least some of the sections covered by the publication, has never looked into these sections without finding much to learn. It is very much to be hoped that the publication will continue for many years to come.

Psychology in Education. By HERBERT SORENSON. London: McGraw-Hill Publishing Co. 1940. Pp. xiv+489. 19*s*. net.

This is definitely intended as a work-book for students. Most of the chapters begin by directions about how to study the topic dealt with, and are followed by Problems and Exercises, References, and Additional References. A very large amount of ground is covered, and as the discussions are largely factual, the specialist will find lots of omissions and not a little dogmatism. It is probable that English psychological educationists who use the book will find it satisfactory only with their better, or more advanced, students, and will prefer to use it in conjunction with critical lectures. At the same time the writing is extremely clear and definite, and where experimental facts are reported in detail the reports are reliable. The author has rather a *penchant* for the individual illustration or story, and this may be all to the good, since it may do something to induce the student to treat his future pupils as individuals and not merely as so many types of reaction.

The Three R's and the Adult Worker. By K. E. NORRIS, with an Introduction by C. E. KELLOGG. Montreal: McGill University (London: Humphrey Milford). 1940. Pp. xiv+213. $2.00.

It would be a good thing if something like this Canadian study could be repeated in this country. Dr Norris was interested in how much of his school-taught material the normal adult retains with sufficient liveliness for it to be of much use to him. He took therefore groups of unemployed men and boys and groups of employed men and school pupils and employed various well-described methods to discover at least some of the answer to his problem. He found on the whole fairly marked gains in some subjects, especially native language, vocabulary, geography and a kind of general

science; but tremendous losses in others, especially in arithmetic, algebra, geometry, chemistry and physics. He also found much disturbing evidence to the effect that many of the better grade pupils leave school early, leaving perhaps less naturally efficient ones to fill the higher school places. And he also discovered many interesting facts with regard to the selective and educative functions of certain occupations. The book could perhaps have been condensed, but it contains very good summaries to help the hurried reader. The research was well worth doing, and ought to stimulate further developments.

Guidance for the High School Pupil. By E. C. WEBSTER, with a Foreword by W. D. TAIT. Montreal: McGill University (London: Humphrey Milford). 1939. Pp. xvi+153. 6*s.* 6*d.* net.

This volume is published in the McGill Social Research Series. After a general discussion of the main problems involved in fitting education and the student to make use of High School training, the author studies in detail: Who goes to the High School, what is his normal Progress through the Grades, and how important it is to make a thorough and controlled study of the Individual Student. He gathers together the results of his survey in a series of clearly stated conclusions, dealing both with modifications which he desires to see in the school system and with the development of methods for assisting the student. The monograph is well-planned and executed, and all those who are interested in practical, but liberal, education in this country have much to learn from it. The final conclusion is one of great interest in this country, for it reinforces what has often been said in England and is based upon a wide and well-attested mass of facts: "The recommendation of school leavers", says the author, "to particular occupational groups can be finally justified only if adequate employment opportunities are available. Without the central organization of placement services, little can be done to help the pupil who drops out of school before completion of his course."

It may be noted that the author pays a tribute to Dr C. S. Myers and the National Institute of Industrial Psychology, and to Dr Angus Macrae in particular, from whom and at which he received both part of his training and much of his inspiration.

Shell Shock in France, 1914–1918. By CHARLES S. MYERS. Cambridge University Press. 1940. Pp. xii+146. 4*s.* 6*d.* net.

This is an account of some of the author's experiences from the outbreak of war in August 1914 to his demobilization in March 1919. It is fortunate, though it must have been exasperating at the time, that doctors over forty were not being accepted, in 1914, for service overseas; otherwise Dr Myers might have found himself entangled in the meshes of the army machine as a Regimental doctor instead of being able, as he was later on, to develop his own special line of interest. After various attempts to obtain medical work with the forces, Dr Myers was eventually appointed to the Hospital unit taken out to France by the Duchess of Westminster. In December 1914 he was gazetted a temporary Captain in the R.A.M.C. and in August 1916, by which time he had risen to be a Lieut.-Colonel, he was appointed Consulting Psychologist to the British armies in France.

Two distinct threads are interwoven in the narrative: (*a*) the recognition of, and the development of the methods of dealing with, the war neuroses; and (*b*) the heart-breaking delays and difficulties in getting anything done.

The historical account of the differentiation of the neurotic from the concussion cases has not, so far as the reviewer knows, been outlined so clearly and fully before and is well worth perusing; while there are two excellent chapters on the features, causation and treatment of 'shell shock' which will interest the general reader and be of value to the medical officer or practitioner who has no previous knowledge of the psycho-neuroses. Other chapters deal with the disposal of the cases in France and England.

The account of the author's dealings with the higher authorities makes mournful reading, especially in these days when the nation is fighting for its life. Fokker, in his autobiography, also complains of the difficulties he, fortunately for us, encountered from the higher command of Imperial Germany in getting improvements in his aeroplane designs accepted. One wonders if *both* sides in the present conflict are still floundering in the same quagmire.

Psychiatric Social Work. By L. M. French. New York: The Commonwealth Fund (London: Humphrey Milford, Oxford University Press). 1940. Pp. xvi+344. 13*s.* 6*d.* net.

This book describes in detail the history and present status, functions and training of psychiatric social workers in the U.S.A. Since the main emphasis is on actual practice, which appears to be extremely variable, it is not of great interest or importance to the English reader. It seems that the American psychiatric social worker may be allowed a considerable freedom in psychotherapeutic practice which the scope of her training and experience do not altogether justify. But there is an interesting discussion in Chap. VI, Some Trends in Social Treatment, of the transition which has occurred in recent years from the more 'active' type of treatment with its prolonged intellectual discussion of difficulties and its forceable manipulation of the environment, to the more 'passive' type in which some effort is made to help the patient and his relations to a real emotional understanding of their difficulties.

The Human Mind. By Murdo Mackenzie. London: J. and A. Churchill. 1941. Pp. vii+215. 7*s.* 6*d.* net.

The book is worth reading even though it seems to achieve formulation and a way of thinking rather than genuine psychological explanation. It begins with a somewhat sweeping statement: "In its early days Psychology was the study of man's behaviour and emphasized his sensations, emotions and instincts rather than his organ of thought. The notion of man's mind as a discrete organ with a specific function, working in rhythm in space and time and driven by strictly mental forces is, I think, new, and elaborated for the first time in this book." It is strange to set this view beside the psychologies of some twenty or thirty years ago which attacked all the earlier psychology for its over emphasis upon ratiocination and its belief in some unique thought function. To Dr Mackenzie the mind is an organ working in space and time,

and following in space the principles of Simplification and Amplification, and in Time those of Immediacy and Deliberation. Into this framework he is able to fit all the principal facts which the psychological physician needs in his attempt to guide unhappy and maladjusted lives into more healthy ways. And it seems to be as good a framework, for all practical purposes, as many others. It has the great advantage, too, of not landing up the physician in deep technicalities and high-faluting phraseology. Dr Mackenzie is able to take instances of most of the better-known abnormalities and to apply to them the general categories of his scheme, and in doing this he has much to say that is well worth noting. He writes with sympathy and much understanding of many cases which have come under his own care. If the whole essay leans perhaps itself towards Simplification—in at least one of its senses—this is no great fault, and indeed is a relief in the general welter of a good deal of current psycho-pathology. When, however, he applies his principles to the social problems created by the modern Dictator and his vagaries, as he does in the final two chapters, the treatment bears signs of hasty preparation and insufficient care.

Psychology, Psychotherapy and Evangelicalism. By J. G. Mackenzie. London: George Allen and Unwin, Ltd. 1940. Pp. xiii + 238. 10*s*. 6*d*. net.

The essay ranges over many fields in psychology, psychopathology, theology and philosophy. Prof. Mackenzie writes with great learning and consistent sincerity. He never shrinks from argument and is not infrequently sweeping and dogmatic. He believes that the psychology of religion must now proceed by the study of particular types of religious experience and he selects evangelicalism for his study because it is perhaps the most outstanding kind of religious bent that is directly based upon first hand experience. "What unites all Evangelicals, indeed enables them to be classed as one type of religious devotee, is an experience which they believe to be of God's grace mediated through an experience of Christ." Evangelicalism, like the psychoneuroses, is the outcome of conflict, perhaps sharp and dramatic, perhaps deep but not salient. But while the psychoneuroses try to avoid the issue, evangelicalism is said to face it and solve it. Hence much of the central part of the essay has to do with conversion. Prof. Mackenzie is convinced that while the solution to life's deepest conflicts lies in evangelical experience, anybody who wishes to help others to this solution will do it all the better with a knowledge of psychology.

Fear may be Liars. By John Ryle. London: George Allen and Unwin, Ltd. 1941. Pp. 95. 3*s*. 6*d*. net.

No doubt there will be many who will be helped and enlightened by the kindly reasonableness of Prof. Ryle's approach to the age-old problems and terrors of death, pain and fear. But the psychologist knows that our feelings about these are inevitably confused by irrational phantasies, desires and actions originating in the unconscious mind, which a rational objectivity can do little to counter. Here lies the importance of the faiths which seem to Prof. Ryle so unnecessary. It is not justifiable for the biologist to dismiss them as futile without taking into consideration their relationship to the other phenomena of the unconscious mind.